OBITUARY NOTICES OF FELLOWS DECEASED.

Sir George Airy was born on July 27, 1801, and died on January 2, 1892. It is not a possible task to compress into a few pages the ninety years' work of a great man; all that can be done is to indicate a few of his many achievements. When his life is written it will be a book and not a pamphlet, and only then shall we understand how much of our scientific knowledge we owe to him. The number of articles and memoirs which he has communicated to the various Societies and journals in which he was interested are over five hundred in number. The first of these is a paper read to the Cambridge Philosophical Society, on November 25, 1822, on the use of silvered glass for the mirrors of reflecting telescopes, and the last is his Numerical Lunar Theory. The date of the first paper is particularly interesting, for it is the year *before* he took his degree. The last paper is also remarkable, for, remembering that the theory of the Moon is one to which some mathematicians have devoted nearly their whole lives, it shows the old man attacking a laborious problem with the energy of youth.

Sir George was educated at private schools at Hereford and Colchester. His vacations from school were spent at the Hill Farm, near Playford, with the uncle by whose assistance he was enabled to go to Cambridge. He never appears to have played cricket, or football, or rowed, but he delighted in pedestrianism. We are told that, with a companion, he once attempted to walk from Playford to Bury St. Edmunds and back in the same day. They reached Rushmere Church on their way home, but could not do the last mile and a half, and the journey had to be finished in a cart sent to meet them.

In 1819 he entered Trinity College, as a sizar, but it does not appear that he was elected a scholar until he was in his third year. In 1823 he became Senior Wrangler and First Smith's Prizeman. The writer can remember that when, thirty years after that date, he entered Cambridge, the story was still told amongst the undergraduates of how wonderfully Airy had answered in the examination. Possibly nothing had been lost in the telling, but there must have been some extraordinary excellence to have attracted such long continued attention.

As soon as he had taken his degree be began a life of ceaseless scientific activity. Memoirs followed each other with ever increasing rapidity, each bearing evidence of much thought and of considerable

work. The subjects also were of the most varied kinds and on all parts of mathematical philosophy. At first he wrote chiefly for the Cambridge Philosophical Society; thus in March, 1824, he calculates the effect which the ring of Saturn produces on his figure. He tries to verify the curious observations of Herschel that this planet is protuberant between the poles and the equator, but he finds that theory leads to an exactly opposite result. The observations of Herschel were, however, considerably modified by those of Bessel some years after. In another paper in the same year he discusses achromatic eye-pieces; for this and his other papers on optical subjects, the Copley Medal of the Royal Society was adjudged to him in 1831. Soon after, he writes on the proper forms of the teeth of wheels, though, owing to the extensive use of iron where wood was formerly used, this subject has no longer the interest it once had.

One of the most interesting papers written by him, about the year 1825, is on a peculiar defect of his own eye and the mode of correcting it. He discovered that in reading he did not use his left eye. Supposing this to be due to habit, he endeavoured to read with the right eye shaded, but found he could not distinguish a letter at whatever distance from the eye the characters were placed. Some time after he made a further discovery, viz., that the image of a point formed by that eye was not circular but elliptical. From this and other appearances he inferred that the refraction of the eye was greater in the vertical plane than in that at right angles. To correct this it would be necessary to use a lens which would refract more powerfully the rays in one plane than those in the perpendicular plane. His idea was that the lens should have one surface cylindrical and the other spherical, and he describes at length his experiments to determine the proper radii. The result was so successful that he was able to read the smallest print at a considerable distance with the left eye as well as with the right. In his subsequent papers he frequently returns to this subject and makes several reports to the Society on the changes produced in his eye by lapse of time.

In 1826, when Ivory criticised Laplace, Mr. Airy, in a paper contributed to the Cambridge Philosophical Society, was not afraid to intervene between two such distinguished analysts when he thought that both had gone wrong. Mr. Airy was not indisposed to controversy; possibly it added a touch of life to his science. We find him afterwards engaged in many disputes, in all of which he was able to prove that he was a tough adversary. In this year, three years after his degree and ten years before he became F.R.S., he read his first paper before the Royal Society. The subject was the much debated question of the figure of the Earth. Alluding in it to the peculiar views of Ivory of fluid equilibrium, he was attacked by that mathematician in a somewhat arrogant manner. This called forth from Mr. Airy a

crushing reply, which he published in the 'Philosophical Magazine' in 1827.

It is, however, impossible even to mention the names of the many papers of which he was the author. The few which have been mentioned above prove how soon after his degree he took a leading part in the scientific work of the day. They show also how, from the very beginning, his mind was turned to practical applications, leaving aside any pure theorems of which he did not see the immediate use.

In 1826 Mr. Airy published his mathematical tracts, which almost immediately became the standard text-book for students in the University. In the first edition we find only the lunar theory, the figure of the Earth, precession and the calculus of variations, the tract on the planetary theory and that on the undulatory theory being added in the second edition, 1831. As his object was to give a clear statement of first principles, he put into the book just what was wanted at the time he wrote, making his judgment with admirable skill. The student world has now outgrown the book, but this is in part due to the excellence of the teaching of the book itself. The first tract, that on lunar theory, is interesting to Cambridge students for another reason. The attention of the University had been so long confined to the works of Newton that the analytical mode of treatment had been almost entirely neglected. The methods of Newton are, Mr. Airy remarks, beautiful, but they have all the imperfections which necessarily accompany first attempts; for the explanation of some of the lunar inequalities they are hardly sufficient, and for the calculation of most they are quite inadequate. For other branches of physical astronomy, such as the planetary theory, their inadequacy has never been questioned. In this tract he endeavours to lay before the student an analytical view of the lunar theory, giving references to the 'Principia' to show the connexion between the different systems. The tract on the calculus of variations is the only one which is purely mathematical. Though it does not go very far into the subject, yet the author must have had a deep sense of the power of this calculus, for he has used it in his physical papers, even in places where simpler methods might more naturally have suggested themselves to his mind. In the preface he speaks of this calculus as the most beautiful of all the branches of the differential calculus. The excellence of the tract on the undulatory theory is evident when we remember the length of time in which it was regarded as the standard text-book of the University. When the other tracts, after a long life, became antiquated, this one retained its popularity, and has been reprinted several times by itself, and is even yet in use.

Mr. Airy was elected a Fellow of Trinity College the year after his degree, and later on in life he was chosen one of the three first Honorary Fellows of the College, the others being Thirlwall and

Tennyson. In 1826 he was appointed Lucasian Professor of Mathematics, but this professorship he soon exchanged for the Plumian, to which he was appointed in 1828. According to the Calendar of that date, his predecessor in office merely gave lectures in the first half of the midsummer term, while those of the former Lucasian Professor are only vaguely referred to. But these were greatly enlarged by Mr. Airy, whose syllabus extends over forty-eight pages of print. They comprise statics, dynamics, hydrostatics, and geometrical optics, but their chief character seems to have been the theory of undulations. Many of the experiments on polarised light whose mathematical theory is given in his tract on the undulatory theory were exhibited here. He appears to have been the first to introduce into Cambridge studies the beautiful theories of Fresnel. With these as subjects, treated in his own skilful manner, we need not wonder at the popularity of his lectures. Even after he had become Astronomer Royal, we learn from his first report to the Board of Visitors, that application to the Admiralty had been made by several members of the University and by the Plumian Professor to allow him to give another course of lectures at Cambridge.

Along with the Plumian Professorship Mr. Airy undertook the duties of the Director of the Observatory. He at once entered on these arduous duties with his usual energy. His efforts were well seconded by the University, who at once raised the slender income of the professorship to an amount nearly double its former value. In the first volume of the 'Astronomical Observations' he tells us that he was induced to fix on a plan of publication very different from that of the 'Greenwich Observations.' He remarks that the value of unreduced observations is so small that to most persons they are absolutely useless. Few, who have not made observations, understand how much time and calculation must be employed before they can be applied to any useful purpose. On the average, the preparatory steps and the observation of a transit occupy from five to ten minutes, while the complete reduction and discussion of the observations employ full half an hour. The professor even said that if an offer was made of a mass of regular meridional observations unreduced, he would not think it worth acceptance. In giving, therefore, the results, he was giving the produce of four or five times as much labour, necessarily irksome, as if he gave merely the unreduced observations. The report for the year 1828 covered the interval of five months' residence at the Observatory; he had no assistant, and every step from making the observations to revising the proof-sheets had to be done by himself alone. Yet in April of the following year the report was published with all the necessary reductions. This promptness is maintained in the succeeding years, and excited the admiration of M. Quetelet, the Director of the Obser-

vatory at Brussels, who says, "Nous sommes à peine au milieu de 1832, et déjà nous possédons les observations de M. Airy, pour toute l'année 1831 : et ce qui peut paraître plus étonnant encore, toutes ces observations sont calculées et discutées avec soin."

It is interesting to observe the care with which he chose the objects to which he should turn his attention as an astronomer, and the constancy with which he stuck to his choice when once made. The chief object, he says, must be such that it could be accomplished by a single unassisted observer, and yet be so important as to be of public use. After consideration he decided that the observations of planets had at that time been so neglected, that one who wished to revise the planetary tables would find himself destitute of the necessary data on which to found his investigation. As soon, therefore, as the Cambridge Observatory was placed under his direction, he made the observation of planets the leading object of his labours. He says in one of his reports that "hardly a single observation of a planet has been lost when the transit was at such an hour that in the regular routine of observations it was practicable to observe it." The wisdom of his choice is shown by the fact that his successor followed closely the same objects. Other pressing wants in astronomy were also present in his mind, and others again rose unexpectedly in the course of his work. In reading his yearly volumes of observations, one notices among other things the care which is taken to secure accuracy. No labour is spared, no calculation is allowed to pass without repeated examination. "To observe all night and to calculate all day" is the description of an astronomer's duties given by an astronomer. In the arrangement of his results, we notice, also, how everything is subordinated to increasing their immediate utility as well as securing accuracy in their details.

When Professor Airy first went to the Observatory the only large instrument was a transit, though this was one of the best of its kind. So energetic an astronomer was not likely to be satisfied with this ; accordingly in 1834 he obtained a large mural circle. In the report for that year he describes the unexpected and annoying difficulties which arose in connexion with that instrument. In the next report we find that these difficulties have been overcome by considering that the effects of the discordance of zenith points on direct and reflexion observations are equal. Later on the great Northumberland equatoreal was added. The establishment to work these was also necessarily increased, and two assistants were given to him.

Perhaps one of the most remarkable examples of Mr. Airy's insight into astronomical questions is his discovery of a new inequality in the motions of Venus and of the Earth. The attention of the Board of Longitude having been directed to the state of the solar tables used in the construction of the 'Nautical Almanac,' he was desired to

examine the discrepancies between the computed right ascensions of the Sun and those observed at Greenwich. On making a comparison between the discrepancies in the position of the Sun's perigee as given by late observations with those given by the observations of the last century, he concludes there must be some yet undiscovered inequality which has been omitted from the calculations. He soon discovered that this originated in the fact that thirteen times the periodic time of Venus is so nearly equal to eight years that the term depending on this phase received a multiplier of more than two millions in integrating the differential equations. On the other hand, the coefficient is of the fifth order with regard to the eccentricities and inclinations of the orbits. In the report on this paper drawn up by Whewell and Lubbock for the Royal Society, it is pointed out that no numerical calculation of a perturbation of the fifth order had been executed, except in the case of Jupiter and Saturn, where, as Laplace states, this labour, "pénible par son excessive longueur," had been performed by Burckhardt; and no calculations of a new inequality of a high order, requiring to be placed in the planetary tables, with a new argument, had been published since that of the great inequality by Laplace in 1784. They conclude by remarking that this is the first specific improvement in the solar tables made by an Englishman since the time of Halley. For this brilliant investigation the Astronomical Society in 1833 awarded to its author their gold medal. The whole of Professor Airy's process was afterwards verified, first by Pontécoulant, and secondly by Leverrier, and found to be correct.

In the years 1831–32, Professor Airy, though so fully employed at the Observatory, was yet able to make some important investigations in the theory of light. Thus he communicates to the Cambridge Philosophical Society a paper to show that the two rays produced by the double refraction of quartz are elliptically polarised. This is soon followed by two or three papers on some phenomena connected with Newton's rings. Just as Sir W. Hamilton afterwards predicted internal and external conical refraction after studying the analytical properties of the wave surface, so Professor Airy discovered these phenomena by using Fresnel's general formula for the intensity of reflected light. When Newton's rings are formed by light polarised in a plane perpendicular to that of incidence between two substances of different refractive indices, and the angle of incidence lies between the polarising angles, the rings should appear white centred, instead of having a central dark spot. Here was a recondite phenomenon which could only be seen when several special conditions were satisfied. Would it be confirmed by experiment? He describes the difficulties of the experiment and its final success. As we read the paper, we perceive how he is led on by slight unexpected discrepancies to

improve the theory. He remarks that there must be a gradual, though rapid, change of phase, instead of the sudden one given by Fresnel's formula, thus seeing faintly a result clearly explained five years after by the theoretical investigations of Green.

At this period of his life Professor Airy's labours are evidently divided between astronomy and the theory of light. The first was connected with his work at the Observatory, the second with his lectures as Professor. Thus, in 1833, he writes in the 'Cambridge Transactions' on Newton's experiments in diffraction; in 1835, on the diffraction of an object glass with a circular aperture; in 1838, on the intensity of light in the neighbourhood of a caustic. In 1840 he chose as the subject of the Bakerian Lecture the theoretical explanation of an apparent new polarity in light.

There is an equally important list of papers on astronomy. In 1832 he communicates to the British Association a report on the progress of astronomy during the present century. This was translated into German, three years after, by C. L. Littrow, of the Royal Observatory, Vienna. The Viennese astronomer thinks that Professor Airy has treated German astronomy like a step-mother, but, nevertheless, he says there is no other work in which the progress of astronomy is so briefly and so accurately given. In 1834 he writes for the 'Nautical Almanac,' on the perturbations of small planets and comets of short period. There is more than one paper on the mass of Jupiter. In 1834 he writes a paper, for the Astronomical Society, on the solar eclipse of July 16, 1833, which was seen extremely well at Cambridge. On this occasion he adopted a new plan of observation; instead of noting the times of the beginning or the end, he so chose the quantities to be measured that any errors in the elements would be observed after they had been largely multiplied. For example, at the beginning of the eclipse, when the discs of the Sun and Moon only slightly overlap, it is obvious that the length of the straight line joining the cusps is much more affected than the versine by any small error in the angular distance of the centres of the discs. To detect such errors, the attention of the observer should be directed to the length of this line. In like manner, the whole duration of the eclipse was divided into periods, for each of which he arranged appropriate measures.

These papers, too numerous to catalogue in this place, did not exhaust the energy of the Professor, for he found time to publish treatises on Trigonometry, the Figure of the Earth, and one on Gravitation. The latter was written for the 'Penny Cyclopædia,' but previously published, in 1834, for the use of students in the University of Cambridge. It was an attempt to explain the perturbations of the solar system without introducing an algebraic symbol. Having thus denied himself the use of the most powerful

engine of mathematics, he expresses his surprise at finding that a satisfactory explanation could be offered for almost every inequality recognised as sensible in works on physical astronomy. The book, though well received, was, for many reasons, not so popular as his tracts. In 1884, however, it received the honour of a second edition.

In 1836–37 he was President of the Astronomical Society. In the first of these years, when presenting the medal of the Society to Herschel for his catalogue of nebulæ and clusters of stars, he gave an interesting account of the history and of the then state of our knowledge of these curious bodies. The next year the address was on the perturbations of comets.

The year 1835 was a great epoch in Mr. Airy's life, for he was then appointed Astronomer Royal. How thoroughly he intended to work the National Observatory is evident from his very first report, for here we find traces of the reforms he intended to introduce; the arrangement of the volumes of observation was to be remodelled; the library improved; a new equatoreal was suggested; a magnetic apparatus had already been acquired, and the site of a magnetic observatory chosen.

Remembering the views he had expressed on unreduced observations when he began work at the Cambridge Observatory, we naturally inquire what he did with the vast mass of ancient observations which he found unreduced when he arrived at Greenwich. This we learn gradually as we read his reports to the Board of Visitors. In 1841 the observations of planets from 1750 to 1830 had already been reduced to longitude and latitude, and every one had been compared with the place computed from the best modern tables. Sufficient time had not yet elapsed to allow of the reduction of the lunar observations, for here 8000 places of the Moon had to be deduced from observation, and 8000 places had to be computed in duplicate from tables exhibiting the complicated results of the most advanced modern theory. In extent and in importance this work may be considered comparable to any that has yet been undertaken in astronomy. In 1846 these lunar reductions were entirely completed. One immediate result was that Hansen discovered two inequalities of long period in the Moon's motion, produced by the attraction of Venus, though some doubts were afterwards thrown on one of these by Delaunay and Newcomb. For these reductions he received in 1846 a gold medal, and in 1848 a testimonial, from the Astronomical Society. Sir John Herschel, in the latter of these years, after noting that this work will remain to the latest posterity a monument of national glory, remarked that we owe to other nations, and especially to the French, the filling up of the great outline struck by Newton with the analytical expressions of the laws of lunar and planetary motion. This glory, he says, they have fairly won, and it is theirs.

"But the broad basis of observations upon which this magnificent superstructure has been reared is British; in the National Observatory it was created. Such has been the mission of that establishment, and such Mr. Airy has wisely judged it must continue to be, to furnish now, and in all future time, in an unbroken series, the best and most perfect data by which the laws of the lunar and planetary movements, as developed by theory, can be compared with observation."

In the report for 1841 he also describes how the Magnetic and Meteorological Department had grown into an important branch of the Observatory. He tells us that the regular work of the establishment is to observe the meridional, bifilar, and horizontal needles, the barometer and thermometers, besides several other instruments, every two hours night and day, except on Sundays; to pursue incessantly the magnetic observations whenever anything unusual occurs; to observe some of the instruments every five minutes during twenty-four hours on a fixed day every month. As the observations, when made, had all to be reduced and tabulated in proper shape, it is clear that the amount of work done must have been very great. Some of these troublesome observations were afterwards abbreviated by a system of self-registration by photography. The description of this new system is given in the volume of 'Greenwich Observations' for 1847.

In April, 1839, Mr. Airy read to the Royal Society his first paper on the correction of compasses. Captain Flinders, in his famous voyage to Australia, had observed that the north end of his compass appeared to be drawn towards the bows of his ship; and he, and others after him, had suggested methods of compensating the cause of the disturbance. The Astronomer Royal was, however, the first to make a thorough investigation of the laws of magnetic disturbance. The iron ship "Rainbow" was placed at his disposal with a view of discovering by experiment some method of controlling the strange deflections of the compass. The use that he made of this vessel will make its name as famous in the history of the mariner's compass as Stephenson's "Rocket" is in the history of locomotives. Assuming that every particle of iron in the ship is, by the action of terrestrial magnetism, converted into a magnet, he calculated the resolved forces on one end of a compass needle whose centre holds a fixed position in the ship. He found that these forces contained two sets of terms, which he called the semi-circular and quadrantal variations, their phases being respectively the azimuth and twice the azimuth of the ship. The latter of these he found to be due to the induced magnetism; while in the former the permanent, the sub-permanent, and the induced magnetism had shares. Having determined the coefficients of these variations by observing the times of vibration of a delicate needle placed first on shore and then on the ship, he was

able to compare the actual and calculated deviations of the compass from the north for all azimuths of the ship. He soon found that almost the whole deviation of the compass was accounted for by the permanent magnetism, and that the residual part followed nearly the quadrantal law. He thence deduced a simple rule by which the compasses could be practically corrected to a first approximation. Putting the ship's length (1) north and south, and (2) east and west, he showed how to place two permanent magnets near the compass so that it indicated true magnetic north in each position. Placing next the ship's length north-east and south-west, the effect of the quadrantal deviation became prominent, and this he corrected by a mass of soft iron, whose own induced magnetism, when properly placed, counterbalanced that of the ship. These corrections being disturbed when the ship heeled over, another magnet was added. On applying this method to the "Rainbow," and trying the compass with the ship's head in different positions through the circumference, it was sensibly perfect; the deviation, which at first had exceeded 50°, sometimes on one side and sometimes on the other, was at once reduced to half a degree. The great development of iron-built ships soon rendered some modification in these corrections necessary; improvements were made by their author, and other mathematicians also made a special study of the deviations of the compass. Mr. Airy wrote several other papers in connexion with this subject, such as those in 1840, 1856, 1860, and 1862, and in 1865 he delivered a course of three lectures at the School of Naval Architecture and Marine Engineering at South Kensington. The principle that the compass ought be corrected by magnets or otherwise has not been universally received; it was contended that it was better to use a table of errors. The Astronomer Royal maintained that the former course was the proper one, while Mr. Archibald Smith has been the champion of the latter. The question has been much discussed, but cannot be entered into here.

Mr. Airy formed one of an important Commission for the restoration of the standards of weights and measures which had been injured by the fire at the House of Commons. Contrary to the opinion prevalent in France, the Commission recommended that the standard measure should be defined by the length of a certain rod preserved in some place of safety, and not by any natural standard, such as the length of the seconds pendulum. Contrary, also, to the method adopted by Bessel for the Prussian standard, the yard is defined by the distance between two points marked on the bar and not by the length of the bar. The history of standards is given by Mr. Airy in a long paper in the 'Phil. Trans.' for 1857.

About 1841 Mr. Airy turned his attention to the theory of tides. He wrote several papers on this subject, discussing separately the

tides in the Thames, at Ipswich, Southampton, the coast of Ireland, and, later on, the tides at Malta. A Royal Medal was adjudged to him by the Royal Society in 1845 for his inquiry into the laws of the tides on the coast of Ireland. His chief work on this subject is his essay on "Tides and Waves," printed in the 'Encyclopædia Metropolitana.' Taking the usual division of the theory into three parts, viz., the equilibrium theory, that of ocean tides, and that of river tides, we may ascribe the initial steps in the first to Newton and Bernoulli, those in the second to Laplace, while the last may be said to have begun with Airy. This important paper, perhaps because it had not been published by any learned society, did not attract the attention it deserved on the Continent, but its merits could not remain unnoticed, and in 1875 the section on river tides was translated and printed in 'Liouville's Journal.' In this section he discusses the broken water seen on the edge of a shoal, why the rise of tide occupies less time than the fall, the solitary wave, the breaking of waves, the effect of the wind, tidal waves, the effect of friction, the form of the wave in broad channels with shallow sides, and other interesting questions. When M. Delaunay, in 1866, suggested that a portion of the apparent acceleration of the Moon was due to a real retardation of the rotation of the Earth caused by tidal friction, Mr. Airy gave a general explanation, founded on the theory of river tides. He discovered two terms of the second order in his equations whose general effect was to produce a constant acceleration of the waters in the direction of the Moon's apparent diurnal course. He therefore gave his entire assent to the views of Delaunay on the existence of one real cause for the retardation of the Earth's rotation. Other causes of retardation have been discovered by mathematicians since then, but these, of course, lie outside the scope of the present sketch.

In the summer of 1844 the arc of longitude between Greenwich and the island of Valentia was measured. As an intermediate station, the longitude of Kingstown was also determined. The difference of longitude was found by making thirty pocket chronometers travel from Greenwich to Kingstown and back again several times; the difference between Kingstown and Valentia being found in a similar manner. The differences in longitude having been found, the next step was to compare the results with the data of the trigonometrical survey, and to see how far they agreed with the best existing determination of the figure of the Earth. The triangulation was then only partially completed, but enough had been done to enable Mr. Airy to arrive at the result that in the latitude of 51° 40′ the length of 1″ in an arc perpendicular to the meridian is 101·6499 feet in terms of the standard bar of the Ordnance Survey. In the same year Struve determined the difference of the longitudes of Altona and Greenwich by the transmission of forty-two chronometers across the German Sea

sixteen times. In the summer of 1862 the longitude of Valentia was again determined, this time by the use of the electric telegraph. The electric telegraph was also used in 1853 to determine the differences between the longitudes of Greenwich, Cambridge, and Edinburgh, and, when the submarine telegraph was laid to Ostend, the difference of longitude between Greenwich and Brussels. These differences of longitude have been combined with other Continental determinations, and, by the use of the whole series, the longitudes of places in the extreme east of Europe may be compared with places in America.

In the year 1844 Mr. Airy also assisted in tracing the Canadian boundary under the Treaty of Washington. The corps of Royal Engineers who were to mark the boundary were placed at the Observatory for instruction and practice in the use of instruments under his eye. The most difficult part of the boundary was a straight line of nearly seventy miles in length, passing through a country of impervious forests, steep ravines, and dismal swamps. He arranged a plan of operations founded on a determination of the absolute latitudes and the differences of longitudes of the two extremities. The azimuths of the line for the two ends were then computed, and marks laid off for starting from each end. One party of engineers, after cutting more than forty-two miles through the woods were surprised, on the brow of a hill, at seeing a gap in the woods, on the next line of hill; this turned out to be the line of the opposite party. On continuing the lines until they passed abreast of each other, their distance was found to be 341 feet. This corresponds to an error of only a quarter of a second of time in the difference of longitudes, and is about one-third of the error which would have been committed if the spheroidal form of the earth had been neglected. This is a striking testimony both to the accuracy of Mr. Airy's method and to the skill of the engineers.

At a special meeting of the Board of Visitors in 1843, Mr. Airy proposed to construct the first of the important instruments he has added to the Observatory. He points out that the Royal Observatory was instituted chiefly to observe the Moon, and that this object had been so continuously kept in view ever since its foundation, that the existing theories and tables of the Moon are founded entirely on Greenwich observations. The unavoidable interruptions to the regularity of the series were, however, so numerous, that the number of complete observations then made was under a hundred a year. He proposed to supply this deficiency by erecting an altitude and azimuth instrument, by which the Moon could be observed in any part of the sky. He assures the Visitors that its cost ought not to be an objection when it is remembered that each complete lunar observation was then worth ten pounds. In the report of 1847 we read that the new instrument had been completed, and was in working

order. In 1848 he proposed to erect a transit circle. Other great instruments followed, and in 1855, when the want of an equatoreal was pointed out, the Board of Visitors warmly supported his views. In the summer of 1860 the instrument was in a state fit for use Enough has now been said to show that by his energy and perseverance the Royal Observatory has been equipped with the most admirable instruments which could be obtained. As early as December, 1849, the Astronomer Royal made an oral statement to the Astronomical Society on the method of observing and recording transits lately introduced in America. He explains how a measure of its accuracy, as compared with Greenwich observations, had been made, and, after pointing out some defects, he thought the possible advantages so great that he contemplated adopting it at the Royal Observatory. In this report for 1854 he tells us that the new barrel-apparatus had been practically brought into use, not, however, without a succession of difficulties, whose causes it was sometimes very hard to discover. He concludes by remarking that the method is troublesome in use, and consumes much time in preparation, but that, amongst the observers who use it, there is only one opinion on its astronomical merits, viz., that, in freedom from personal equation and in general accuracy, it is very far superior to the observations by eye and ear.

In 1846 Mr. Airy was one of three Commissioners appointed by the Queen to report on the proper gauge for railways. The Commissioners considered that the chief advantage of the broad gauge lay in the speed of the trains, while in the conveyance of goods and the cost of outlay the narrow gauge was the superior. For these and other reasons they recommended that the present narrow gauge should by statute be that of all railways to be constructed in future. After examining and rejecting several ingenious inventions by which the same carriage could be made to run on both gauges, they also recommended that some equitable means should be found by which the railways then on the broad gauge could be reduced to the narrow. This second recommendation was not, however, adopted by the legislature. In 1858 Professor Airy was one of the Commission on the Ordnance Survey appointed to consider, amongst other questions, the scales on which the maps were to be constructed.

In the 'Monthly Notices' for November, 1846, there is a memoir by the Astronomer Royal, giving his own, Professor Challis', and Mr. Adams' separate accounts of the discovery of Neptune. Mr. Airy remarks that in the whole history of astronomy there is nothing comparable to this discovery; Uranus, Ceres, Pallas, and other planets were discovered by observation, but, in the case of Neptune, mathematicians stated beforehand that a planet would be found exactly in a certain place and presenting exactly a certain appearance. In that

place and with that appearance the planet was found. The predictions of Adams and Leverrier differed by only one degree of longitude. The controversy which has arisen on this discovery cannot be briefly discussed, and must be omitted in a sketch as short as the present one.

In the years 1850, 1851, Mr. Airy turned his attention to antiquarian researches. There are several papers in the 'Athenæum' on the Exodus of the Israelites, and some more on the place of the landing of Cæsar. The first of these lines of enquiry led gradually to the "Notes on the earlier Hebrew Scriptures" (1876), and the latter to the "Treatise on the Roman Invasion of Britain" (1865). Halley, reasoning on the phenomena of the tides as described by Cæsar, and comparing these with the Channel tides as then known, had concluded that Deal was the landing place. Mr. Airy, however, showed that, with fuller knowledge of the local tides, this line of reasoning would prove that Pevensey was the actual landing place. He also contended that this result was confirmed by a study of Cæsar's movements in Gaul before the crossing, and his transactions in the interior of Britain after the passage of the straits. Mr. Airy was also interested in several other antiquarian questions. Thus in the 'Phil. Trans.' for 1853 there is a paper on the eclipses of Agathocles, Thales, and Xerxes, in which he arrived at some new dates for these events. After the publication of Professor Adams' theory of a diminished value of the acceleration of the Moon's mean motion, Mr. Airy repeated his calculations, and somewhat modified his results. He also compared the dates of thirty-six eclipses given in a Chinese historical work called 'Chun Tsew' with those calculated by theory by a French writer, and points out how generally accurate the Chinese records are on these points.

Mr. Airy had the pleasure of viewing three total eclipses of the Sun, the first from the Superga, near Turin, in 1842, the second at Göthenburg, in Sweden, in 1851, and the third at Hereña, in Spain, in 1860. In the many accounts which he has given of these eclipses, he continually dwells on the impressiveness and awfulness of the scene, pointing out that no degree of partial eclipse gave the least idea of a total eclipse. He mentions, on the authority of Arago, that the officers of a French corvette who had been trained to observe the eclipse of 1842 lost their discipline when the darkness came on, and the observations were not made. The most remarkable of all the appearances at the first eclipse were the red mountains or flames seen round the Moon. It was afterwards discovered that these had been seen in 1733 by a Swedish astronomer, but all the observers at Turin were taken by surprise. It was difficult then to decide what they were, or even whether they belonged to the Sun or to the Moon. In the eclipse of 1851 special attention was given to these flames, and,

in his report to the Astronomical Society, Mr. Airy said it was impossible to see the changes in their aspect without feeling the conviction that they belonged to the Sun and not to the Moon. Still many doubted, but in 1860 the observed angular displacements and velocities of displacements of these red appearances as the Moon's disc passed over the Sun proved decisively that they belonged to the latter body. On the expedition to Spain, the Astronomer Royal was accompanied by a large body of observers; some were trained astronomers, the others amateurs. All did good service, there was much work to be done and it was well done. The Government placed the large steamer "Himalaya" at the disposal of the party to carry them to and from their destination, and in conferences on the deck of that ship the different classes of observation were divided amongst those present. All the details of the organisation of the expedition rested in great measure on the Astronomer Royal, and in his report to the Board of Visitors in 1861 he pronounced the enterprise to have been very successful. In his lecture to the Astronomical Society he remarked that it would be advantageous to collect from the various accounts, first, all the facts which relate to one part of the phenomena, secondly, those which relate to another, and so on; and, finally, to arrange these in separate chapters in order that a systematic comparison might be made. In the forty-first volume of the Memoirs of the Astronomical Society, edited by Mr. Ranyard, these comparisons are published, and occupy 792 pages. The mere titles of the chapters are sufficient to show the importance and interest of the work.

At the meeting of the British Association at Manchester in 1860, Mr. Airy delivered a lecture on the solar eclipse of that year to an assembly of perhaps 3000 persons. The writer of this sketch remembers the great Free Trade Hall crowded to excess with an immense audience whose attention and interest, notwithstanding a weak voice, he was able to retain to the very end of the lecture. This lecture he repeated at Cambridge, as the Rede Lecture, in 1864, where it was again well received. It was afterwards translated into Dutch by D. Bierens de Haan about 1877. The charm of Professor Airy's lectures lay in the clearness of his explanations. The subjects also of his lectures were generally those to which his attention had been turned by other causes, so that he had much that was new to tell. His manner was slightly hesitating, and he used frequent repetitions, which, perhaps, were necessary from the newness of the ideas. As the lecturer proceeded, his hearers forgot these imperfections and found their whole attention rivetted to the subject matter. On many occasions Mr. Airy has given successful lectures. In March, 1848, he delivered six lectures at Ipswich on Astronomy, which were first taken down by shorthand writers and, after correc-

tion by their author, published in a collected form. This treatise has been very popular and has gone through several editions. He also lectured in the Town Hall at Neath; in 1851 at the Royal Institution, on the total solar eclipse of that year; and in 1853 on the eclipse of Thales. In 1878 he lectured at Cockermouth on the probable condition of the interior of the earth. Besides these there were many other lectures, some of which will be mentioned further on.

At his Rede Lecture at Cambridge in 1864 Mr. Airy took occasion to point out what appeared to be defects in the system of education as connected with mathematical physics, and he followed up these remarks by a letter to the Vice-Chancellor. To assist in remedying these deficiencies he had already written, in 1861, his now well-known treatise on the theory of Errors of Observation. With the same object he published, in 1866, his "Partial Differential Equations," in which he introduced the novelty of giving stereoscopic views to illustrate the surfaces under consideration. These were followed by a treatise on Sound in 1868. In order to direct the attention of the University to the subject of magnetism, he gave a course of lectures in the Easter term of 1869, at Cambridge. These being attended by crowded audiences, were developed into the treatise on Magnetism which appeared in the following year.

One of the most remarkable of Mr. Airy's investigations is that in which he determined the mean density of the Earth. He had always been much interested in this subject, and in the fourteenth volume of the Memoirs of the Astronomical Society we find that he assisted Mr. Baily in his important repetition of the Cavendish experiment by contributing the theory and the formulæ. In 1826 another method had occurred to him which promised to give a more accurate result than either Maskelyne's method of measuring the attraction of a mountain or the Cavendish experiment. His immediate object was to compare the force of gravity at the surface and at the bottom of a deep mine, using the pendulum as the means of observation. In the 'Phil. Trans.' of 1856 he describes the attempts he had made at Dolcoath in 1826 and in 1828, and their failure on both occasions in consequence of accidents, once by fire and once by water. Twenty-six years passed before he was in a position to repeat the experiment for the third time. In 1854, however, a new power was, he tells us, placed in his hands. The galvanic system had been established at the Royal Observatory, and, in the familiarity which he now possessed with telegraphic applications, he perceived that the difficulty of comparing the upper and lower clocks would be entirely removed. The experiment was conducted at the Harton Pit, a mine about 1260 feet deep. The result was that gravity below was greater than that above by 1/19286th part, so that the mean density of the earth was 2·7 times the surface density and 6·6 times that of water.

The value thus obtained is larger than that given by the Schehallien method, and considerably larger than that deduced by Baily from the torsion rod experiments. After the experiments at Harton Pit were concluded, he gave a lecture at the Central Hall, South Shields, on the pendulum experiments, and, in the next year, a Friday Evening Lecture at the Royal Institution, in London, on the same subject. These experiments were also noticed by "Mr. Punch" in a copy of verses on "Airy and the Coal Hole," written by Shirley Brooks.

In 1867 Professor Airy read a paper at the Institution of Civil Engineers on the use of the suspension bridge with stiffened roadway for railway and other bridges of large span. This paper was the result of a discussion with Mr. R. Stephenson on the method to be adopted for the Britannia bridge, and the author thought there were better methods available for wide crossings than simple tubular bridges. Besides writing this paper, for which he received the medal of the Society, he often attended the meetings and joined in the discussions.

As the management of the expeditions to observe the transits of Venus in 1874 and 1882 would necessarily fall on the Astronomer Royal, Mr. Airy began his preparations as early as 1857. In that year, he called the attention of the Astronomical Society to the means available for correcting the measure of the Sun's distance in the next twenty-five years. On this occasion he pointed out the peculiar advantages of observing Mars for that purpose, showing that at the opposition of 1860 that planet would make a near approach to the Earth. The preparations for the transit were continued during the following years; the proper places to which the expeditions should be sent were discussed at great length and finally chosen. The northern stations having been occupied by Continental observers, the southern ones were, by the advice of the Board of Visitors, divided amongst the English parties. It was also decided that photographic should be combined with eye observations, and an extra grant was obtained from the Treasury for that purpose. By 1873 preparations had so far advanced that an efficient body of observers from all classes, naval, military, and civilian, were collected at the Royal Observatory, and were being instructed and practised in all the practical details of observation with the transit, the altazimuth, the equatoreal, and especially with the working model of the transit. At this time the received measure of the Sun's distance depended on the transits of 1761 and 1769, but mainly on the latter. Though there was a close accordance in the results obtained from the different transits, yet all investigators expressed doubts on their correctness. In the transit of 1761 the results depended almost entirely on an accurate knowledge of the differences of the longitudes of very distant stations. In that of 1769 the result rested in great measure on the observations of a single person, viz., those of Father Hell at

Wardhoe. Another difficulty had also presented itself; it was found that after the planet appeared to have fairly entered upon the Sun's disk it was for some time connected with the Sun's limb by a black ligament. Even after the planet had separated itself from the edge, it did not immediately assume a circular appearance. It is clear that the determination of the precise moment of contact is more difficult than it was expected to be at the time of the invention of this method by Halley. For these reasons the results of the transit of 1874 were looked forward to with the greatest interest. The observations were on the whole successful, though some perplexing difficulties arose. At several places the black ligament was not seen, but a faint thread of light of sensible width, due to the atmosphere of Venus, presented itself. This unexpected appearance introduced an element of uncertainty as to the exact moment of contact. Two hundred and sixteen photographs were taken, but their examination did not realise all that had been hoped for from them. On the return of the several expeditions, the reduction of the observations was proceeded with under the scrutiny of Captain Tupman. The immense labour of these calculations delayed the publication of the complete results until June, 1881, nearly seven years after the event, though a preliminary report was made to the House of Commons in 1877.

In the 'Phil. Trans.' for 1872 there is a curious determination of the magnetic state of the iron in the Britannia and Conway tubular bridges. This investigation was suggested by the peculiar tremor of the iron felt by the hand when a train was passing. Both Sir George Airy and Mr. Stephenson expressed this by saying that the metal seemed to be in a state of "molecular shiver." As all experiments show that iron in this state of tremor is peculiarly subject to the inductive action of external magnetic force, Sir George thought that observations made along the axis of the tube might lead to some important conclusions. One general result was that "in the axis of a rectangular tube, at all points except very near the ends, the action of external magnetic forces in planes normal to the axis is absolutely destroyed." Some strange anomalies were observed in one tube which could not be explained until it was remembered that that tube had had a fall while being raised into its place. Thus the effects of an accident were discovered a quarter of a century after it had occurred by a magnetic experiment.

The system of time signals by which Greenwich time is spread over the country by means of the electric telegraph is in great part due to Sir G. Airy. It should be noticed that the whole system is automatic; the Greenwich clock being once set each morning to the exact time, the signals are distributed without any person having to touch the apparatus.

It is impossible to consider in detail the numerous additions which Mr. Airy has made to our knowledge at various times. It is necessary to pass over with merely a mention such an important memoir as that in which he discusses theoretically the stresses in the interior of a rectangular beam. In another paper he mentions a method of correcting the chromatic dispersion of the atmosphere in observing transits of Venus. He has also written several papers on the comparison of Earth currents and magnetic disturbances, on the diurnal and annual inequalities of terrestrial magnetism, and on some lunar magnetic inequalities. We must hasten on to his last work.

In 1874 the Astronomer Royal brought before the notice of the Astronomical Society a new mode of treating the lunar theory. After giving a rapid survey of the methods hitherto employed, he remarks that the nature of the steps has compelled the investigators to decide the succession of their terms, not by numerical magnitude, but by algebraical order, and that this has produced great inequality of convergence. The mental labour cannot be alleviated by an amanuensis, and he quotes a remark of M. Plana, "Quelquefois ces calculs me font presque perdre la tête." He proposes, as a new method, to begin with Delaunay's final numerical expressions for the longitude, latitude, and parallax with a symbolical term attached to every number for contingent correction. These corrections are so small that it is sufficient to retain only their first power. The expressions are then substituted in the equations of motion with the time for independent variable, and the result of the substitution is a great number of equations for determining the numerical values of a great many small quantities. In this theory the orders of the terms are numerical and equally accurate throughout; the details are so easy, that a great part can be intrusted to a mere computer. Though he was then seventy-three years of age, he had aleady begun the work. He says that, though it is sufficiently possible that he may not be able to complete it, he desires to have it in such a state that a successor may be able to take it up. For this reason in each of the following years he gives further details as to the theory, and describes how far he had advanced in the approximations. Finally, in 1886 the numerical lunar theory was given to the world in the form in which it was left by the author; a wonderful monument of what a man can do at the age of eighty-five. He explains in the preface how the work was delayed by the heavy pressure of business, not only in the ordinary conduct of the Observatory, but also in completing the calculations for the transit of Venus in 1874, and in preparing for that in 1882. He then remarks on some serious discordances which remain to be accounted for; "I cannot conjecture," he adds, "whether I may be able to examine sufficiently into this matter." He never was able.

A long catalogue of the honours and titles which Sir G. Airy

received from the universities and other scientific bodies is given after his name in the List of Fellows of this Society. He was D.C.L. of Oxford, LL.D. of Cambridge and Edinburgh, one of the first members of the Senate in the University of London. He was one of the eight foreign Associates of the Institute of France, and received the Lalande Medal. He was made K.C.B. in 1872. In 1875 he received the freedom of the City of London, enclosed in a gold casket. He was a Fellow of the Royal Society for fifty-five years, received both the Copley and Royal Medals, and was made President in 1871. He was on the Council of the Astronomical Society for more than fifty years, was five times President, and received two gold medals. He was President of the British Association at the meeting at Ipswich in 1851. He was an honorary member of the Institution of Civil Engineers, and received the Telford Medal. He was elected a foreign Associate of the Dutch Academy of Sciences in 1878, and held honorary titles from many Continental and American Societies. He also received the Albert Medal of the Society of Arts, which was presented to him by the Prince of Wales. He was a member of the Royal Society of Edinburgh and of the Royal Irish Academy. He received a diamond snuff-box from the Emperor Nicholas of Russia, and decorations from the Emperor of Brazil. He was Chevalier of the order "Pour le mérite" of Prussia; he belonged to the Legion of Honour of France, and had decorations from Sweden. A gold snuff-box was given to him by the Steam Navigation Company, a silver-gilt inkstand by the River Dee Company, and he held a French Sèvres vase as a Commissioner on Standards.

When Sir George Airy retired from the Observatory at the age of eighty, the Board of Visitors recorded in their Proceedings a resolution expressing, in an emphatic manner, their sense of his eminent services throughout the long period of forty-five years during which he had presided over the Observatory. Among his many services to science they especially mentioned the following:—(*a*) The reorganisation of the Observatory; (*b*) the designing of instruments of exceptional stability and delicacy; (*c*) the extension of the means of making observations on the Moon in such parts of her orbit as are not accessible to the transit circle; (*d*) the investigation of the effects of iron ships on compasses; (*e*) the establishment of time signals. Turning next to his labours in departments of science not directly connected with the Royal Observatory, they called attention to the high estimation in which his contributions to the theory of the tides, to the undulatory theory of light, and to various abstract branches of mathematics are held by men of science throughout the world.

During his residence at Cambridge Sir George married Richarda, the eldest daughter of the Rev. Richard Smith, of Edensor, in Derbyshire. Lady Airy died in 1875, after a long illness. After his

retirement Sir George received a pension from the Government, and resided close to Greenwich Park, and not far from the scene of his former labours. Here, on his ninetieth birthday, he held his last reception. It was attended by a numerous and distinguished company, among whom was one old friend even older than himself.

Sir George owned a cottage at Playford, near Ipswich, to which he often retired for rest and recreation. Here he had spent his boyhood, and here at last he was laid in the grave by the side of his wife. Visiting the village every year, he could remember five generations of more than one family, and could give the early history of most of the others. The last scene of his life may be said to have begun and ended here. On his last visit he had a fall, which in his enfeebled state proved more serious than might have been expected. He never properly rallied, but gradually sank and passed away at his residence in Greenwich. His funeral at Playford Church was quiet and simple, fitting the noble simplicity of his life.

E. J. R.

Edmond Becquerel.—In the long and brilliant roll of physicists of whom France may justly boast, few names deserve a more prominent position than that of Becquerel. The Becquerel family constitutes a true dynasty of *savants*, and affords a twice repeated instance of father and son engaged in the self-same studies, and seated side by side in the same identical section of the Academy of Sciences of Paris.

The second of the family, Edmond Becquerel was born in 1820, and grew up, as remarked in the official *éloge* delivered by M. Duchatre on the occasion of his death, in a scientific atmosphere. From his illustrious father, Antoine César Becquerel, he evidently inherited his acute power of observation, and that "infinite capacity for taking pains," which seems to be the essential characteristic of the Newtons, the Faradays, the Darwins, and, in short, of all the great leaders of science.

While thus born a *savant*, the cause which determined Edmond Becquerel to become a physicist must be sought in the example and the society of his father Antoine, first as a pupil, then as assistant. Under different circumstances he might have become equally eminent as a chemist or as a student of the organic sciences.

As he approached manhood, the discovery of photography burst upon the world, and naturally drew general attention to the chemical action of light. Edmond Becquerel found here his opportunity, and studied with zeal and success the conditions and laws of the novel phenomena. His papers on the chemical radiations accompanying solar and electric light, and on their effects, were of distinguished value. He even succeeded in obtaining a photographic repro-

duction of the spectrum in its natural colours, with the serious drawback that the coloured image could be preserved only in complete darkness. Our advances upon Becquerel in this direction have not been so rapid or decisive as might have been hoped.

Becquerel came to the conclusion that all the effects produced under the influence of light are due to one and the same radiation acting upon different bodies. He considered it probable that in different beings the retina is not always sensitive between the same limits of refrangibility. His principal researches in this direction are to be found in the 'Annales de Chimie' for November, 1848.

It must not be forgotten that Becquerel's view was strongly opposed by Professor Forbes, of King's College, also by R. Crouch, of Polperro, who both maintained that no marine animal has the power of vision under the influence of such rays of light as would not excite the optic nerve of man, and that those depths of the ocean at which an everlasting darkness prevails, are the regions of silence and death.

It need scarcely be said that recent research has fully proved that existing animal species can recognise rays of light which make no impression on the human retina, thus confirming the correctness of Becquerel and refuting his critics.

It is not surprising that with the researches just mentioned he combined a prolonged and exhaustive study of phosphorescence as produced by insolation.

For the better study of these phenomena, he invented his well-known phosphoroscope. By means of this instrument, substances can be viewed immediately after having been exposed to the light of the sun; the interval between insolation and observation being capable of reduction at pleasure and of very accurate measurement. Edmond Becquerel investigated also the light emitted by the phosphorescent compounds of uranium, and turned his attention to the infra-red portion of the spectrum, and to the ultra-violet portion, to which the chemical action involved in the new art of photography was due. He formed a pure spectrum on a sensitive plate of silver prepared by the process of Daguerre, and found that the fixed dark lines of the solar spectrum as recognised by Wollaston, Fraunhofer, and Brewster were traceable in the chemical impression, and that similar lines therefore existed in the parts of the solar spectrum lying beyond the limits of visibility. Becquerel published in the 'Bibliothèque Universelle de Genève' for 1842 a diagram of the fixed lines of the spectrum as enlarged by these researches. These photo-chemical and optical studies, taken conjointly, have been pronounced by Fizeau to be a model of research in experimental physics.

Another research of the highest order was his investigation of the action of magnetism on all substances, especially upon oxygen, the magnetic function of which he carefully scrutinised. Here he and

Faraday were working simultaneously almost on the same lines. In a letter to the elder Becquerel, Faraday (1851) speaks in terms of high admiration of Edmond Becquerel's achievements. He was exceedingly struck, he writes, with the beauty of the experiments, with the accuracy of the determinations, and with the results, which confirmed and extended his own.

Edmond Becquerel was profoundly interested in the laws of electro-chemical decomposition, his views agreeing for the most part closely with those of Faraday. What is now known as "Becquerel's Law" is a special application of the general law of Faraday governing electro-chemical decomposition.

Among what may be called Becquerel's minor papers are included researches on the thermic radiation of electric sparks, on the production of phosphorescence by electric sparks, on the determination of high temperatures, on the radiation of solar heat and the production of electric currents, on the electro-chemical decomposition of water, on constant current batteries, on the chemical rays accompanying those of light, on the constitution of the spectrum, on the laws of the evolution of heat during the passage of electric currents through solids and liquids, on coloured rings produced by the deposition of metallic oxides on metals, on the electric conductivity of solids and liquids, on phosphorescence produced by insolation. Altogether, E. Becquerel produced seventy-one memoirs which are mentioned in the Royal Society's index, in addition to a number written in conjunction with his father, Antoine César, and afterwards with his son, Henri.

Having thus briefly summarised his scientific work, we may glance at what must be called the outer events of his life. It is to be remarked that though he had early distinguished himself by scientific works of high value, and as the son of an eminent and much respected Academician he was not without influence, yet none of the great scientific establishments of his country offered him an appointment.

In 1849, under the Second French Republic, the Government of the day created a National Agronomic Institute, and adopted the "eminently liberal principle" of appointing the professors by competition. On this occasion, strange to say, the result was favourable. Becquerel, we are told in his official *éloge*, "submitted himself to this redoubtable test," and, for a wonder, though weighed in a balance so little capable of appreciating his merits, he was fully successful. He thus obtained a chair at this important institution, which had been established at Versailles. Here his lectures were fully appreciated by an audience who followed his teachings with lively interest. His career at the Agronomic Institute was, however, of short duration. In two years political events, which need not concern us, brought on the suppression of the Institute. But as one door was

closed, another and a wider opened. Becquerel was called to a chair at the Conservatoire des Arts et Métiers. Here his eminent talents found ample scope.

The Academy of Sciences meanwhile was eagerly looking forward to receive him to its membership. However, on account of strict limitation of its numbers, this inclusion did not become possible until 1863, when the death of Despretz opened the way.

In 1878 he succeeded his father as Professor of Physics at the Museum, where he was duly admired for profound mastery of his subject, as well as for the admirable clearness of his teachings. This important position he continued to occupy down to his death.

Here he was in a position to contribute powerfully to the advance and the diffusion of his favourite science, by his personal researches which he still carried on with unremitting zeal and interest, and by his teachings at the Conservatoire and the Museum.

Becquerel's health was unbroken, notwithstanding intense application to his duties and his studies. Not merely his personal friends, but the learned world, hoped that, like his father, he might reach an advanced age. Such, however, was not to be his lot. In the beginning of May, 1891, he was seized with an indisposition, which, without apparent cause, rapidly intensified, so that a few days saw the end of his brilliant career.

Edmond Becquerel's scientific services can best be judged from a survey of his researches. It will be seen that he took up physics, especially electricity, from what may be called the chemical side. Some of the present developments of the science would probably appeal to him less strongly.

The personal qualities of M. Edmond Becquerel aided in no common degree the attainment of his high position as a *savant*. He was patient, persevering, and truthful, and he is known to have been what the French expressively call *un homme de cœur*.

It is interesting to note that M. Henri Becquerel has been recommended by the Academy of Sciences as the most suitable candidate to succeed his distinguished father in the chair of Physics applied to Natural History, at the Museum.

W. C.

THOMAS STERRY HUNT was born in Norwich, Conn., on September 5, 1826, of an old New England family. His ancestor, William Hunt, was one of the founders of Concord, Mass., in 1635. His maternal grandfather, Consider Sterry, of Norwich, was a civil engineer and mathematician, and was the author of text-books of arithmetic and algebra, published 100 years ago. Mr. Hunt was destined for the profession of medicine, but, after preliminary studies, his love for chemistry and mineralogy led him, early in 1845, to become a special student, and

afterwards assistant under Professor Benjamin Silliman, sen., in Yale College. Two years later, after working for a short time in the Geological Survey of Vermont, he was selected, on Silliman's recommendation, by Mr., afterwards Sir William, Logan to be the chemist and mineralogist to the then recently-organised Geological Survey of Canada. In this position he remained as the colleague and assistant of Sir William for more than 25 years, till his resignation in 1872. His work in this capacity is well known. He was employed in the earliest scientific investigations of the petroleum, the rock salt, the phosphates, and the iron and copper ores of Canada, and also in researches on the composition of a great number of rocks and minerals, of mineral waters, and of soils; while he devoted a large amount of attention to the structure and composition—at that time so little known—of the ancient crystalline rocks of the Ottawa Valley and the Great Lakes, in unravelling the stratigraphical intricacies of which Logan and his assistant Murray were at the same time actively and most successfully occupied. He thus had an important share in the great work of instituting the Laurentian and Huronian systems of Geology, and in systematising our knowledge of the oldest rocks of Canada and of the world. This work he afterwards followed up independently, in the development of the Norian, Montalban, Taconian, and Kewenian systems, in which he included various groups of ancient rocks between the Laurentian and the Cambrian; and, though some of these groups may be regarded as still in dispute, there can be no question of the great scientific value of Hunt's studies of them, and of the new facts which he contributed to their discussion.

While connected with the Geological Survey, Hunt willingly aided in the drudgery of literary work and administration, for many parts of which his early culture and extensive range of reading and knowledge well fitted him.

At this time also he conceived and published, in a succession of papers, those wide views on chemical and general geology which were embodied in his greater works, and more especially in his 'Mineral Physiology and Physiography' (1886), in which he discussed, with a power and range of knowledge rarely equalled, the original condition of our planet, and the genesis of its more ancient rocks, as well as the processes of decomposition, recomposition, and metamorphosis to which they have been subjected. This great and eminently suggestive work deserves the careful study of all concerned in petrography or physical geography, who, whether or not they may agree with all its conclusions, will find very much to instruct, and to stimulate and guide thought and investigation. This work alone, with the earlier essays on chemical geology, would be sufficient to form the basis of a great reputation, and must retain its place as a leading

authority on the subjects of which it treats. As the author himself states, this work, and more especially the "Crenitic" hypothesis developed in it, are the "result of nearly 30 years' studies, having for their object to reconstruct the theory of the earth on the basis of a solid nucleus, to reconcile the existence of a solid interior with the flexibility of the crust, to find an adequate explanation of the universally contorted attitude of the older crystalline strata, and at the same time to discover the laws which have governed the formation and the changing chemical composition of the stratiform crystalline rocks through successive geologic ages."

To Dr. Hunt we thus owe some of the earliest attempts to subdivide and classify in a scientific manner the stratiform crystalline rocks; a work to which he brought not only his studies throughout Canada and the United States, but also the result of inquiries conducted during repeated visits to the British Islands and to Continental Europe. In pursuing these studies, and while reviewing and controverting various hypotheses, including the igneous or plutonic, the metamorphic and the metasomatic, all of which he rejected as irreconcilable with observed facts, and as violating chemical theory, Dr. Hunt vindicated what he deemed the essential soundness of the still imperfect Wernerian aqueous view, and advanced, as its proper development and completion, his own crenitic hypothesis. According to this theory, the source of the various groups of crystalline rocks was "the superficial portion of a globe, once in a state of igneous fusion, but previously solidified from the centre. This portion, rendered porous by cooling, was permeated by circulating waters, which dissolved and brought to the surface during successive ages, after the manner of modern mineral springs, the elements of the various systems of crystalline rocks. These rocks thus mark progressive and necessary changes in the mineralogical evolution of the earth."

Dr. Hunt never abandoned the scientific pursuit of chemistry and mineralogy. In the former science he summed up the general conclusions of his researches in 1887, in his work entitled 'A New Basis of Chemistry,' which has gone into a third edition, and has been translated into French. His latest work, published in 1891, 'Systematic Mineralogy,' gives a new classification of the mineral kingdom, based on an improvement of what used to be called the Natural History System followed long ago by Möhs and Jameson. It would be premature to express any opinion as to the acceptance by chemists and mineralogists in general of the new views propounded in these works; but they are unquestionably able, and full of important generalisations and suggestions which must make their mark in the science of the future.

Dr. Hunt found time to do some work as an educator. He was

Professor of Chemistry in the Laval University, Quebec, from 1856 to 1862, during which time he delivered annual courses of lectures in French. He continued to be Honorary Professor until his death. He was also for several years Lecturer in M'Gill University, Montreal, and was Professor of Geology at the Massachusetts Institute of Technology, 1872–78. Among his academic titles were those of M.A., Harvard; Sc.D., Laval; LL.D., M'Gill; and finally LL.D., Cambridge, England. He was elected a Fellow of the Royal Society of London in 1859. He was a member of a large number of other societies, both Canadian and foreign. A member of the National Academy of Science of the United States, in 1873, he was President of the American Association for the Advancement of Science and of the American Institute of Mining Engineers, and twice President of the American Chemical Society. He was one of the original members and the third President of the Royal Society of Canada, which, uniting some of the features of the British Association with those of a Royal Society, elects a new President annually. One of the organisers of the International Geological Congress, he was its first Secretary, and was a Vice-President at the Congresses of Paris, 1878, Boulogne, 1881, and London, 1888. In connexion with the great industrial exhibitions, Dr. Hunt represented Canada as a member of the International Juries at Paris in 1855 and 1867, and at the Philadelphia Centennial Exhibition in 1876. He was an officer of the French order of the Legion of Honour and of the Italian order of St. Maurice and St. Lazarus.

In 1878 Dr. Hunt retired from public professional life, and devoted himself mainly to the perfecting of his more important works in new editions, and to the preparation of his 'Systematic Mineralogy.' His health and strength, however, gradually declined, and, continuing to work almost to the last, he passed away peacefully on Friday, February 12, 1892. His death must be deplored as a great loss to science; but he had the good fortune, not granted to all scientific workers, to have means and leisure in his closing years to bring together in a complete and elaborated form all the principal scientific results of the work of his life.

In 1878 Dr. Hunt married the eldest daughter of the late Mr. Justice Gale, a lady of culture and literary taste, who survives him.

J. W. D.

Carl Wilhelm von Nägeli, the son of a country doctor, was born on March 27, 1817, at Kilchberg, a village overlooking the Lake of Zurich. His school days were for the most part spent at the Zurich Gymnasium, and at their close he entered the University of Zurich with the intention of preparing for his father's profession. His inclinations soon led him, however, to pursue scientific and philosophical

studies, rather than medical, and to devote himself more particularly to botany. With this special object in view, he studied for some time under Aug. Pyrame de Candolle at Geneva. He graduated at Zurich in 1840 with a botanical dissertation, the subject of which was the Swiss species of the genus *Cirsium*. Within a short time of his graduation he visited Schleiden, then Professor at Jena, and, coming under the influence of his powerful personality, Nägeli threw himself heart and soul into the microscopic researches which, in the hands of von Mohl and Schleiden, were quickening and developing botanical science. The first fruits of his labours in this direction were his "Beiträge zur Botanik," short papers on various subjects, published in 'Linnæa,' 1842; his important paper "Zur Entwickelungsgeschichte des Pollens" (1842); and his remarkable contributions to the short-lived 'Zeitschrift für wissenschaftliche Botanik' (1844–46).

In 1845 Nägeli married. On his wedding tour he spent some time on the south-west coast of England, where he continued the algological studies begun on a visit to Naples in 1842, the outcome of which was his important work 'Die neuern Algensysteme,' published in 1847. Shortly after this Nägeli, who had for some years been a "Privat-Docent," was promoted to be "Professor extraordinarius" at Zurich. During the next few years he did not publish much beyond a valuable work on fresh-water Algæ ('Gattungen einzelliger Algen') in 1849. He was not by any means idle, however, for he was engaged on various researches, in the prosecution of which he associated with himself, in the year 1854, his pupil Carl Cramer, and which were published in 1855–58, forming the monumental 'Pflanzenphysiologische Untersuchungen.' In the meantime Nägeli had become Professor of Botany at Freiburg-im-Breisgau (1852), and had passed on from there to Munich (1857). Of the work of the ten succeeding years, his fundamental anatomical studies were published in his "Beiträge zur wissenschaftlichen Botanik" (1858–68); the rest he contributed to the 'Sitzungsberichte' of the Bavarian Academy of Sciences, of which he continued to be an active contributing member until late in life. The very numerous papers which he read before the Academy, dealing with a variety of subjects belonging to all departments of botanical science, form three volumes of 'Botanische Mittheilungen' (1861–81). Special mention should be made of his important paper, "Theorie der Gährung," published (1879) in the 'Abhandlungen' of the Bavarian Academy. During this period he also wrote 'Das Mikroskop,' in conjunction with Schwendener (1867, 2nd ed., 1877); and works on the lower Fungi ("Die niederen Pilze in ihren Beziehungen zu den Infectionskrankheiten und der Gesundheitspflege," 1877; also in 'Untersuchungen über niedere Pilze; aus dem Pflanzenphysiologischen Institut in München,' 1882). With the publication of his great work, 'Die

mechanisch-physiologische Theorie der Abstammungslehre,' in 1884, his scientific career may be said to have closed.

Nägeli was ennobled by King Ludwig II in 1878, and was elected a Foreign Member of the Royal Society in 1881. He continued to hold the Botanical Chair at Munich until his death, at the age of seventy-four, on May 11, 1891. His health, already impaired by advancing years and excessive work, had been further weakened by an attack of influenza in 1890, from the effects of which he had never completely recovered.

Most of the foregoing details as to Nägeli's life have been taken from the appreciative notice in 'Nature' of October 16, 1891, by Dr. D. H. Scott, who obtained them from the funeral oration (in 'Neue Zürcher Zeitung,' May 16, 1891) delivered by Professor Cramer, Nägeli's whilom colleague.

With Carl von Nägeli disappears the last member, and not the least distinguished, of that triumvirate of workers who, half a century ago, were so largely instrumental in laying the foundation of the scientific botany of to-day. Of his two coadjutors, Hugo von Mohl, the creator of an intelligible vegetable histology, died in 1872 at the age of sixty-seven; and Matthias Jacob Schleiden—who, though so much of his own work has become obsolete, rendered invaluable service in his relatively short botanical career (1837—1850) by insisting on the study of development as the only basis of a sound morphology, and by inspiring enthusiasm for research in this direction—survived until 1881, having attained the age of seventy-seven.

It is impossible, in the case of a prolific worker like Nägeli, to touch, however briefly, on all or nearly all the important contributions to knowledge which he made. It must suffice to dwell on such of them as may be regarded as epoch-making; but of these there are many, for it has fallen to the lot of but few scientific workers to discover so many fundamental facts as Nägeli did.

If, as appears probable, the "Beiträge zur Botanik" constitute Nägeli's first attempts at research in the field of the new botany, it cannot be maintained that his *début* was altogether brilliant. The reason of this comparative failure seems to be that, inspired probably by personal enthusiasm for his master, he undertook these investigations with the object of supporting the Schleidenian theory of cell-formation, and of defending it against the well-grounded criticisms of von Mohl and of Unger. The actual work is good, even remarkable for the time, but most of the conclusions, and many even of the observations, are vitiated by the only too obvious *parti pris*. It is not until he has regained something of his independence of thought that Nägeli's genius begins to manifest and assert itself. This is clearly observable in the almost contemporaneous paper on the development of the pollen, where he asserts (p. 17) that the

cell-wall is formed not immediately round the nucleus (cytoblast), as Schleiden taught, but round a granular mass in the midst of which the nucleus lies. In this paper also he incidentally corrected the erroneous view that the granules in the contents of the pollen-grain represent spermatozoids, and he observed in certain cases the presence of two nuclei in the grain, though he failed to appreciate the full meaning of the fact. But his comprehension of the nature of the cell was still far below the high level which is marked in his memorable paper "Zellenkerne, Zellenbildung und Zellenwachsthum," which appeared in the 'Zeitschr. für wiss. Botanik.' In that paper his chief objects are the extension of Robert Brown's discovery of the nucleus (1831) to the principal families of Cryptogams, and the investigation of the processes of cell-multiplication. With regard to the former point, he arrives at the wide generalisation that the nucleus is present in all classes and orders of plants, and that its absence from any plant-cell has not been, and probably cannot be, proved; and he also recognises that the nucleus is not a solid mass, but a vesicle, with a proper membrane enclosing fluid granular contents with one or more nucleoli ('Zeitschr. für wiss. Botanik,' Heft 1, 1844, pp. 68 *et seq.*). With regard to the latter point, he gives up the universality of the Schleidenian theory for which he had so stoutly contended, and admits (*loc. cit.*, Heft 3 and 4, 1846, p, 49) that, in vegetative organs at least, cell-multiplication is effected by division, "free cell-formation" being restricted to the reproductive organs.

The chief cause of Nägeli's change of view on the subject of cell-formation was an interesting discovery which, he tells us, he made whilst investigating Algæ at Naples in the summer of 1842. He recognised that the "mucus" in the cell forms a continuous lining to the wall of healthy cells, to which he gave the name "Schleimschicht" (*loc. cit.*, Heft 1, p. 91). It is true that Kützing had, in the previous year ('Linnæa,' 1841), made a similar observation, also in the Algæ, but he had failed to interpret it correctly: he termed it "Amylidzelle," thinking that it was, or could be changed into, starch. Nägeli, on the contrary, ascertained that this layer consists of granular "mucus," which at an earlier stage more or less fills the cell-cavity, and at a later stage forms a parietal layer; and further, that its reactions show it to be nitrogenous. Having discovered this, he felt that the results of his two earlier papers were no longer convincing, since the phenomena which he had regarded as the expression of processes of free cell-formation could be ascribed, with greater probability, to the contraction of the "Schleimschicht" away from the cell-wall. Subsequently (*loc. cit.*, Heft 3 and 4, 1846, p. 52) he carried this discovery to its final stage, asserting that the "Schleimschicht" is never absent from living cells, that it is, in fact, itself living, and that it is from it and by it that the non-nitrogenous cell-wall is formed at its surface.

It is well worthy of note that this subject was at the very same time engaging the attention of von Mohl. The year in which Nägeli announced his discovery of the "Schleimschicht" was also the year in which von Mohl described the "primordial utricle" ('Bot. Zeitg.,' 1844). Similarly, the year in which Nägeli published his more complete account of the physiological importance of the "Schleimschicht" was the year in which von Mohl arrived at the comprehensive conception of this living substance under the name of "protoplasm" ('Bot. Zeitg.,' 1846. See also Nägeli, "Primordialschlauch," in 'Pflanzenphysiologische Untersuchungen,' 1855).

But other treasures lie buried in the now almost forgotten pages of the 'Zeitschrift für wissenschaftliche Botanik.' Heft 1, which was entirely written by Nägeli, contains, besides the first part of the great cell-paper, an interesting account of the remarkable Siphonaceous Alga, *Caulerpa*: a philosophical essay on the fundamental conceptions of Botany; and. finally, a paper with the title "Bewegliche Spiralfaden (Saamenfaden?) an Farren." This paper announces, in fact, the discovery of the antheridia and spermatozoids of Ferns. Although, from the facts of their development, their structure, and their chemical reactions, Nägeli rightly regarded the actively moving, spirally coiled filaments as the equivalents of the spermatozoids of the Mosses and other Cryptogams, still he expressed himself only tentatively on this point, since he had failed to discover the female organ, and had therefore been unable to observe the process of fertilisation. Consequently this discovery remained incomplete until Leszczyc-Suminski detected the female organs on the prothallia of Ferns (1848), and Hofmeister established the homology of these female organs with those of the Mosses, and called them by the same name, "archegonia" ('Bot. Zeitg.,' 1849: 'Vergl. Unters.,' 1851).

In Hefte 3 and 4 of the same periodical, Nägeli published some observations on the Rhizocarps ('Ueber die Fortpflanzung der Rhizocarpeen;" also his criticism of Mettenius, on p. 293) which extended to this group the discovery previously made in the Ferns. He points out that the microspores (at that time known as "pollen-grains") of *Pilularia*, and by analogy those of its allies, do not, as asserted by Schleiden, grow out into pollen-tubes which effect fertilisation as in Phanerogams, but that they give rise to free-swimming spermatozoids. Here also he failed to discover the female organ, borne on the prothallium ("Keimwulst"), developed by the macrospore (then called the "embryo-sac"); and this is the more strange, because he actually saw and figured the neck of the female organ sufficiently clearly to be able to correct Schleiden as to the mode of fertilisation in these plants, pointing out that what Schleiden had taken to be a group of germinating pollen-grains were nothing more than the rows

of cells constituting the neck of the organ which Hofmeister afterwards proved to be an archegonium.

The stage of Nägeli's work has now been reached at which he more especially devoted his energies to the study of the Algæ; and a brief sketch of the results attained in this direction will not be out of place in view of the general biological importance of some of his observations, and of the basis which they afforded for his investigation of other families of plants. The 'Zeitschrift für wiss. Botanik,' in addition to incidental observations on Algæ in some of the other papers, contains four which are exclusively algological: these are "*Caulerpa prolifera*" (Heft 1); "Wachsthumsgeschichte von *Delesseria hypoglossum*" (Heft 2); "*Polysiphonia*" and "*Herposiphonia*" (Hefte 3 and 4). The first of these is of importance in that it gave an insight into the structure, not only of this remarkable plant, but also of the whole family, the Siphoneæ, of which it is a highly-developed representative; it proved that here the whole plant, though attaining a considerable size, and exhibiting unmistakable morphological differentiation into stem, leaf, and root, consists of a continuous mass of protoplasm, unsegmented by cell-walls, and may be termed "unicellular," as Nägeli termed it, though subsequent investigation has shown it to be multinucleate. The paper on *Delesseria*, though of but slight algological interest, is, however, really epoch-making, in that it gives the first account of growth by means of a single apical cell, a discovery which was confirmed by his subsequent observations on *Polysiphonia* and *Herposiphonia* (see also "Wachsthumsgeschichte" of *Pterothamnion* and of *Hypoglossum Leprieurii*, in 'Pflanz. Physiol. Unters.,' Heft 1, 1855). His papers on these two Algæ are of peculiar importance, in that they manifest a clear recognition of the fact that (see especially p. 220, and Plate VII, figs. 1, 2, 3), in the Florideæ at least, the cell-walls are porous, and that the cytoplasms of adjacent cells are connected by protoplasmic filaments passing through the pores of the intervening walls; demonstrating, in fact, that "continuity of protoplasm" which has been rediscovered and studied during the last ten years. The algological interest of these two papers is also great, for they contain some of the earliest observations on the antheridia of these plants, on the tetraspores, and on the cystocarps (as they had been termed by Kützing), which Nägeli considered (see Heft 1, p. 47) to be receptacles for gemmæ, as in *Marchantia*, and consequently termed "Keimbehälter." At this period he does not express any opinion as to the sexuality of the Florideæ; the first definite statement on the subject, unfortunately quite erroneous, occurs in "Die neuern Algensysteme" (1847), where he separates the Florideæ from the rest of the Algæ (including herein the Lichens) on the ground that the latter are destitute of sexual organs, whereas the former possess them in

the form of male organs (antheridia) and female organs, "in which, as a rule, four spores are developed." For several years Nägeli continued to hold the view that the mother-cells of the tetraspores are the female organs of the Florideæ; thus, in his paper, "Beiträge zur Morphologie und Systematik der Ceramiaceæ" ('Sitz.-ber. d. k. B. Akad. d. Wiss.,' 1861; also 'Bot. Mittheil.'), he says, with reference to this view, that up to that time he had seen no sufficient reason for changing it. And yet, in this very paper, he gives an excellent description, with good figures, of the development of the "Keimfrucht" of *Callithamnion*, an organ which, but a few years later ('Mém. de la Soc. des Sci. Nat. de Cherbourg,' vol. 12, 1865; 'Ann. d. Sci. Nat., Botanique,' sér. 5, vol. 7, 1867) Bornet and Thuret proved, by observing the process of fertilisation, to be the true female organ (procarp).

The discovery in the Algæ of the growing-point with an apical cell was probably the inducement which led Nägeli to study the growing-point in other groups of plants. He first described the apical cell and the mode of growth of the stem in various Mosses and Liverworts ('Zeitschr. f. wiss. Bot.,' Heft 2), and subsequently ("Ueber das Wachsthum des Gefässstammes," *loc. cit.*, Heft 3 and 4), he discovered that in *Lycopodium* and in Monocotyledons and Dicotyledons, the growing-point of the stem has no apical cell, but consists of a small-celled merismatic tissue, in which he traced the first indications of the differentiation of the permanent tissues. From this he went on to the fundamental anatomical work which constitutes the main portion of the 'Beiträge zur wiss. Botanik,' in the form of three papers: (1) "Das Wachsthum des Stammes und der Wurzel bei den Gefässpflanzen, und die Anordnung der Gefässstränge im Stengel" (Heft 1, 1858); (2) "Dickenwachsthum des Stengels und Anordnung der Gefässstränge bei den Sapindaceen"; and (3), in conjunction with Leitgeb, "Entstehung und Wachsthum der Würzeln." The last of these papers was for years the authoritative work on the anatomy and growth of the root: it is true that Hofmeister ('Vergl. Unters.', 1851) had already discovered the fact that in many of the Vascular Cryptogams the growth of the root is effected by a single apical cell, but this detracts but little from the value of Nägeli's work. This paper also contains the recognition of the morphological peculiarities of the "rhizophores" of *Selaginella*.

The papers in the 'Pflanzenphysiologische Untersuchungen' which call for special notice are that on the "Primordialschlauch" (Heft 1, 1855), and that on the "Stärkekörner" (Heft 2, 1858). The chief importance of the former is the light which it throws on the physics of the cell, with special reference to the influence of the living 'primordial utricle" on diosmose, a subject which was also engaging the attention of Pringsheim ('Bau und Bildung der Pflanzenzelle,' 1854),

These contemporaneous papers brought into notice those phenomena which, under the comprehensive term "plasmolysis," have come to be of such importance in the explanation of the mechanism of plant-movement. The "Stärkekörner" is a paper which most strikingly illustrates in how remarkable a degree Nägeli combined untiring industry with scientific insight. Taken simply as a descriptive account of the starch-grains, of the variety of their form in different plants, of their structure, and of their chemical composition, it is the most exhaustive that has yet appeared. The preparation of it, which began in 1850, involved, as Nägeli tells us in the preface, the microscopic examination of the roots and rhizomes of about 800 species of plants, and of the seeds of about 1700 species. But the main interest of this paper lies, not so much in the facts, as in the theories concerning the mode of growth and the molecular structure of organised bodies, for which the facts afforded the necessary basis: and further, in the recognition of the importance of the appearance and disappearance of starch-grains as an indication of the metabolic activity of the plant.

In 1846 ('Zeitsch. f. wiss. Bot.,' Hefte 3 and 4, p. 117) Nägeli had expressed the opinion that a starch-grain is a vesicle the wall of which becomes thickened, like that of a cell, by the deposition of successive internal layers. This view, which he had formed, he says, whilst "in Irrthümern der Schule befangen," he now replaces by the well-known theory of "growth by intussusception." This theory, which Nägeli extended also to cell-walls, was generally accepted for many years, until the publication of Strasburger's researches ('Bau und Bildung der Zellhäute,' 1882) when a reaction took place in favour of the theory of growth by apposition. At the present time, however, Strasburger admits ('Histologische Beiträge,' Heft 2) that the growth of cell-walls may be effected by the infiltration of material, a process which, as he says, may be termed "intussusception," but is not altogether the same as that conceived and named by Nägeli, which was inseparably associated with his theory of the intimate structure of organised bodies, generally known as the "micellar theory." According to this theory, organised structures, such as cell-walls and starch-grains, consist of solid ultimate particles which Nägeli at first regarded as being the chemical molecules, but subsequently ('Das Mikroskop,' 1877, p. 354), as groups of molecules termed "micellæ," each of which is, under ordinary circumstances, surrounded by a layer of water. The particles with their watery envelopes are, he argued, held together by the mutual attraction of the solid particles, the attraction of each particle for its watery envelope, and the cohesion of the ultimate chemical molecules of which each particle consists. By acute reasoning based on the study of the physical properties of organised structures, more especially

their "swelling-up" and their behaviour with polarised light, Nägeli arrived at the conclusion that the micellæ are biaxial crystals, and assigned to them as a probable form that of parallelopipedal prisms with rectangular or rhomboid bases. On this theory of their structure, the growth by intussusception of cell-walls and starch-grains would consist essentially in the stretching of the growing layer so as to widely separate the existing micellæ, and in the intercalation of new micellæ into the intervening watery areas.

Brilliant as was this attempt to give a purely physical explanation of certain biological phenomena, and helpful as it undoubtedly was for a time, it must now be confessed that it was inadequate, and that it is to no small extent responsible for the tendency to regard plants more as inanimate objects than as living organisms, which for many subsequent years impeded rather than assisted the advance of knowledge as to the mechanism and physiology of the growth and other movements of plants.

Incomplete as this notice is, and indeed must be, it would be altogether inadequate were it not to include some account of Nägeli's attitude with regard to evolution, a subject upon which he was peculiarly qualified, both by his philosophical habit of mind and his observations as a naturalist, to express a weighty opinion. Throughout his life he was an enthusiastic field-botanist, and acquired a special familiarity with the remarkable phenomena of distribution presented by the flora of his native Alps. With the view of giving precision to his field-observations, he devoted particular attention to the large and highly variable genus *Hieracium*, the thoroughness of his study being attested by a number of papers on the genus in the 'Botanische Mittheilungen,' and by a monograph 'Die Hieracien Mittel-Europas,' prepared in conjunction with Peter, of which the first volume ("Piloselloiden") was published in 1885, whilst the second ("Archieracien") was not completed at the time of his death.

The first definite statement of Nägeli's views on evolution appears in an address delivered before the Bavarian Academy, on March 28, 1865, entitled "Entstehung und Begriff der naturhistorischen Art," which is to a large extent a criticism of the 'Origin of Species.' Beginning, with characteristic completeness, at the origin of living organisms, Nägeli emphasises the importance of Lamarck's assumption that the simplest organisms were, and are continually, formed by spontaneous generation. Going to the consideration of Darwin's theory of evolution by natural selection, Nägeli regards it as being based essentially on the principle of utility, "die Nützlichkeitstheorie ist der Darwinismus"—a principle which he considers to be quite inadequate to explain the facts of the phylogeny of either plants or animals. Darwin's theory, which involves the assumption that variation takes place indeterminately in all directions, both higher

and lower, requires, so Nägeli thought, to be modified and supplemented by the "Vervollkommnungstheorie," inherited from Lamarck, according to which variation takes place determinately and in the higher direction only. From this point of view, Nägeli emphatically asserted that the degree of variability which gives rise to more or less constant varieties or races is the result, not of external, but of internal, causes, an assertion which has since been strongly supported, though on different grounds, by Weismann and others.

The whole matter is fully discussed in his 'Mechanisch-physiologische Theorie der Abstammungslehre,' published nearly twenty years later, where, though there is greater elaboration, there is no essential change of view. In this work Nägeli introduces the idea of a material basis for heredity in the form of what he terms "idioplasm." The characters which an organism inherits from its parent or parents are transmitted by the idioplasm of the reproductive cell or cells, in which all the properties of the adult form are inherent. But the idioplasm of any one generation is not identical with that of either its predecessor or its successor. It is always increasing in complexity as regards both the arrangement and the constitution of its micellæ, with the result that each generation marks an advance, though not always an immediately appreciable one. But the variation which is primarily due to the inherent properties of the idioplasm is not altogether unaffected by external conditions; when these act continuously in a given direction they may cause modifications which are, however, merely adaptive. In a word, whereas, on the Darwinian theory, all organisation is essentially adaptive, according to Nägeli the development of higher organisation and of more complete division of labour is the visible expression of the spontaneous evolution of the idioplasm.

Although Nägeli's theory of evolution, as laid down in the 'Abstammungslehre,' has not met with general acceptance, more particularly as regards the small importance which he attaches to natural selection, still the work as a whole cannot be judged as other than a worthy close to the labours of its author. Full as it is of the accumulated knowledge and experience of a long and earnest scientific career, it constitutes one of the most valuable and suggestive recent contributions to the theory of reproduction and descent.

S. H. V.

PHILIP HERBERT CARPENTER, the fourth son of Dr. W. B. Carpenter, was born at Westminster, on February 6, 1852. He received his earlier education at University College School; and in 1871 commenced residence at Cambridge as a scholar of Trinity College. He graduated B.A. in 1874, in the First Class of the Natural Sciences Tripos, and proceeded to the further degrees of M.A. in 1878, and D.Sc. in 1884.

After graduation he spent some time in the University of Würzburg, working under Professor Semper; and in 1877 he was appointed Assistant Master at Eton College, in special charge of the teaching in biology, a post which he held up to the time of his death, on October 22, 1891.

Herbert Carpenter was a naturalist from the first, and took part in some of the most important biological movements of recent times. He accompanied the earlier dredging expeditions which preceded and rendered possible the famous cruise of H.M.S. "Challenger;" he was a prominent member, and one of the earliest, of the new school of biology at Cambridge; and his appointment on the staff of one of the greatest of public schools was hailed as a significant recognition of the educational value of biological science, and of the importance of making adequate provision for it in schools.

When only sixteen years of age he accompanied his father on the deep-sea exploring expedition of H.M.S. "Lightning." In the following years he took part in the dredging cruises of the "Porcupine," and in 1875 he was one of the scientific staff on board H.M.S. "Valorous," which accompanied Sir George Nares' Arctic Expedition as far as Disco Island, for the purpose of sounding and dredging in Davis Strait and in the North Atlantic.

On these expeditions he was mainly occupied in chemical and physical observations, and it was not until he went to Würzburg, in 1875, that he commenced the special study of the group of animals, the Echinodermata, to which the remainder of his life was so successfully devoted. The choice of this group was almost an accidental one, and was determined in the first instance by the opportunity his residence at Würzburg gave him of examining Professor Semper's specimens, and so of determining the real extent and cause of the discrepancies between the results obtained by his father and by Professor Semper, with regard to certain points in the anatomy of the recent Crinoids.

Professor Semper was much interested in Carpenter's work, and on the completion of his first paper, placed in his hands the important collection of Actinometræ he had himself obtained from the Philippine Islands. The examination of this material occupied Carpenter for nearly two years, and led to the publication of an elaborate and very important monograph on the genus, in the Transactions of the Linnean Society, which established his reputation as an authority on the group. On the return of the "Challenger" Expedition, and the distribution of the collections, the free-swimming Crinoids were at once entrusted to Carpenter, and on the death of Sir Wyville Thomson, in 1882, the stalked Crinoids were handed to him as well. Carpenter's reports on these two groups, published in 1884 and 1888 respectively, are of a most elaborate and complete

character, and rank among the most valuable contributions ever made to our knowledge of the group.

During the preparation of the '"Challenger" Reports,' and after their completion, Carpenter was continuously engaged in the study both of Crinoids and of other groups of Echinodermata, and the long list of his published papers bears striking evidence to his industry and enthusiasm. He did not himself work much at the embryology of the group, but he paid very special attention to the fossil members: indeed he stands out prominently as one of a comparatively limited number of zoologists who have given practical and emphatic recognition to the fact that zoology and palæontology must not be taught or thought of as separate sciences, or be worked at by separate investigators, but must be considered and treated as one great branch of science. "I have," he says, "the strongest conviction (and many mistakes would be avoided were it a universal one), that the only way to understand fossils properly is to gain a thorough knowledge of the morphology of their living representatives. These, on the other hand, seem to me incompletely known if no account is taken of the life-forms which have preceded them." It is to this thorough-going recognition that fossils are not merely parts of animals, but parts of animals akin to those now living on the earth, that the special value of Carpenter's palæontological work is due. Perhaps his most important contribution from this point of view is the admirable catalogue of the Blastoidea in the British Museum, of which he is joint author with Mr. Robert Etheridge, jun.: many of his smaller papers on palæontological subjects are also of great value.

As a zoologist, Carpenter was admittedly the leading authority on the group he had made so specially his own. He was enthusiastically devoted to his subject, and always ready to show his specimens and discuss his results with any brother naturalist. His own work is characterised by extreme thoroughness and conscientiousness rather than by brilliancy, and is perhaps for this reason more certain to endure. As a teacher he never had more than imperfect opportunities; but, working under circumstances in many ways discouraging, he achieved very considerable success; and amongst his pupils are some who have gained marked distinction in the science they first learned to know and to respect through him.

He was a kindly, generous, and unassuming man, whose untimely death will be mourned by friends in many nations.

A. M. M.

SIR WILLIAM CAVENDISH, seventh DUKE OF DEVONSHIRE, Knight of the Garter, was born on April 27, 1808. His father was Mr. William Cavendish, who married Louisa, daughter of the first Lord Lismore. His grandfather, Lord George Augustus Cavendish, third son of the

fourth Duke of Devonshire, was created Earl of Burlington in 1831. The Cavendishes have been conspicuous in English history for several centuries, but here it is only necessary to note the late Duke's connexion with two of the most eminent philosophers this country has produced. His great-grandmother, wife of the fourth Duke of Devonshire, was the Lady Charlotte Boyle, daughter of the Earl of Burlington and Cork, and the direct descendant of Richard, second Earl of Cork, who was brother to the Hon. Robert Boyle, the celebrated chemist and natural philosopher of the 17th century. His great-grandfather was first cousin to Henry Cavendish, the no less celebrated chemist and natural philosopher of the 18th century. The late Duke was educated at Eton, and at Trinity College, Cambridge, and his career at the University was exceptionally brilliant. In the Mathematical Tripos of 1829 his name appeared as Second Wrangler, Philpott, lately Bishop of Worcester, who survived him only a few days, being the Senior Wrangler. At the ensuing examination for the Smith's Prizes, the order of their names was reversed, and they both appeared in the First Class of the Classical Tripos. In the same year he married Lady Blanche Howard, daughter of the Earl of Carlisle; and in the following year he was returned as colleague of Lord Palmerston, to represent the University in Parliament. It might have been expected that the descendant of the staunch friend of Lord William Russell would side with the Whigs; he was not, however, a man to be guided merely by tradition, or to take his opinions at second hand, but was Liberal by conviction, and had the courage of his opinions. In the debates on the Reform Bill, which ensued almost immediately after his return to Parliament, he spoke in favour of the measure, and in a few thoughtful and wise words pointed out the dependence of good government upon the confidence and support of the people. This was too much for his academic constituency, and at the next election he and his colleague lost their seats. He continued, however, in the House of Commons for a year or two, representing first Malton, and then Derbyshire, until in 1834 he succeeded his grandfather as Earl of Burlington.

In the Upper House he very rarely spoke. It cannot be said that he took little interest in politics, for he was a keen observer of the course of events, and formed a shrewd judgment of their issues; but he never laid himself out for debating. He was too conscientious, too cautious of using idle words, ever to be a ready speaker. The condition of Ireland, where the just claims of his tenants were never forgotten by him, gave him great concern; and he was a consistent supporter of all measures for bettering the condition of the people, and removing the grievances which alienated them from England. Yet he dissented on principle from Mr. Gladstone's method of dealing

with Home Rule in Ireland, and became the chairman of the Loyal and Patriotic Union.

Although so little prominent in politics, he never shrank from coming forward whenever his influence, his counsel, or his wealth could be used to advance the interests of the community. In the extension of education, among all classes, and in all aspects, he was, perhaps, best able, as well as most willing, to take the lead. He was Chancellor of the University of London, from 1834 to 1856; and, on the death of the Prince Consort, in 1861, was elected without opposition Chancellor of the University of Cambridge. This office he filled until his death. That University, in 1861, was beginning to enlarge its bounds and widen the range of its teaching, and he not only watched its progress with never-failing sympathy, but contributed to it very materially by his noble foundation of the Cavendish Laboratory, over which Clerk-Maxwell presided. As Chancellor he had sometimes difficult questions to solve, sometimes personal matters requiring care and tact to decide; yet the interests of the University never suffered in his hands, and his decisions were invariably accepted by all parties. He looked, however, far beyond the limited class who can graduate at Cambridge, and when the movement was started for carrying University teaching into the large towns, he cordially supported it.

He was President at one time of the Owens College, at Manchester, and a generous benefactor both of it and of the Yorkshire College, at Leeds. After the foundation of the Victoria University, he became its first Chancellor. The importance of science in relation to the industrial progress of the nation he highly appreciated, and the report of the Royal Commission on Technical and Scientific Instruction, over which he ably presided, has contributed in no small degree to the awakening of public attention to that subject. On the formation of the Iron and Steel Institute, he became its first President. To agriculture he was always devoted. He was one of the founders of the Royal Agricultural Society, and in 1869 its President; and with his accustomed generosity he contributed to the foundation of the Agricultural College at Cirencester. Quite recently, at the instance of the Minister for Agriculture, he moved the University of Cambridge to take steps for promoting the study of scientific agriculture.

He managed his own estates, and they could hardly have been better managed, for he always thought of his tenants' interests. When, some thirty years ago, agricultural rents were everywhere rising, he made no changes in his agreements, and the result was that when the depression came he lost neither tenants nor rents. But he was never content merely to leave things as he found them. For him it was a duty to the country to develop its resources and

improve the condition of the people. A large part of the valuable hematite iron ore of the Furness district lay on his estate, and Barrow, which in thirty years has grown from a small village to a town of over 50,000 inhabitants, with its docks and its ironworks, owes its existence and its prosperity in great measure to his sagacity in enterprise, and to the liberality and earnestness with which he carried out his plans. In a different way, but in the same spirit, he promoted the building of Eastbourne.

His own life was of the simplest. Most of it was spent at Holker, near Grange, in Lancashire. There he took part in the ordinary business of the county and neighbourhood, and for fifty years was chairman of the Board of Guardians for the Poor. He lost his wife in 1840, but the education of his children was his personal concern. To train his sons to take their place in the State, and to watch their careers, was his especial delight. It will be understood how acute must have been to him the suffering when his second son, Lord Frederick Cavendish, Chief Secretary for Ireland, was murdered in the Phœnix Park. His third son, Lord Edward Cavendish, also predeceased him, but only by some months. He passed away peacefully and painlessly, in his eighty-fourth year, on December 21, 1891. Rank, wealth, intellectual gifts, had no power to affect the simplicity of his character, or lessen the deep sense of duty which controlled all his actions.

G. D. L.

James Risdon Bennett was the son of a learned dissenting minister, the Rev. James Bennett, who preached for many years at Falcon Square, in the City. His mother was a descendant of Risdon Daricott, the Evangelist of Somerset in the middle of the eighteenth century.

He was educated first at Sheffield and afterwards at the University of Edinburgh, where he took his degree of Doctor of Medicine in 1833, in his twenty-fourth year. After this he travelled for a time on the Continent, and had some thoughts of settling for practice in Rome. He returned, however, to London, and, after a short connexion with Charing Cross Hospital, was appointed Assistant Physician to St. Thomas's Hospital, which was then still occupying the site of the old monastery and sick refuge of St. Thomas in the street in Southwark which still retains that name. Here, Dr. Bennett afterwards became full Physician and Lecturer on Medicine. He had previously joined the hospital at Victoria Park for diseases of the chest, which had been founded in 1848.

He was much interested in the physical diagnosis of affections of the heart and lungs, and was proficient in stethoscopy when this branch of clinical medicine was still little known and even slighted as a French innovation.

Meantime, Dr. Bennett had married and taken a house in Finsbury Square, where he obtained considerable family and consulting practice, and formed the friendship of Dr. Jeafferson, Dr. Gull, Dr. Peacock, and Dr. Herbert Davies, who were then his neighbours.

In 1875 he was elected a Fellow of the Royal Society, and in 1876, after serving several offices in the College of Physicians, he was chosen to fill the highest official position in his profession by being made its President. In the presidental chair, Dr. Bennett was at his best. Courteous and dignified, patient and intelligent, never pompous and never weak, he showed on every occasion his good judgment, good temper, and good sense.

On his retirement, after five years service, he received the well-earned honour of knighthood, and always retained the esteem and confidence of the College. He had no pretention to oratory, but his clear-headed and pithy remarks were always listened to with attention. At the Council of the College and on the Council of the Royal Society he was valued for his friendliness, his sagacity, and his power of silence as well as of speech.

Sir Risdon Bennett took an active part in the preparations for receiving the International Congress of Medicine which met in London in 1881; and, as Chairman of the Reception Committee, welcomed the visitors in a French speech which was delivered with remarkable vigour, and was understood as well as applauded by all present, including the Frenchmen.

He had moved to Cavendish Square in the year 1876, and died there of gradual but rapid senile decay at the ripe age of eighty-two.

He was a man of tall and dignified presence, and pleasant, unaffected manner. Strictly upright and honourable in his professional and private life, he was fond of conversation and society, a good talker and a good listener, with no trace of envy in his composition, and a cordial recognition of the merits of his compeers. Otherwise happy in his family, he sustained a heavy loss in the death of his eldest son, who, after a promising career at Cambridge, had only lately been called to the Bar. With this exception, Sir Risdon Bennett's course was one of uninterrupted prosperity. His piety was deep and unobtrusive, and led him to devote much time and labour to philanthropic work. He was well read in general as well as medical literature, but published little—an essay on Hydrocephalus, written when he was a young man, the Lumleian lectures on thoracic tumours, and a few cases in the medical journals—one published only a year before his death. Perhaps his most characteristic appearance in print was a paper advocating counter-irritation as a mode of treatment, which appeared in the 'Practitioner.'

His high character, his sagacious judgment, and the unaffected sincerity of his convictions secured for him general respect and esteem,

and he deserved the praise due to those who pursue a liberal profession in a liberal spirit and who form a link between practice and science.

P. H. P. S.

Sir Edward Sabine was the fifth son and ninth child of Mr. Joseph Sabine, of Tewin, Herts. The Sabines were originally of Norman extraction, and were settled in Kent, at Patricksbourne, until the beginning of the eighteenth century, when Joseph Sabine migrated to Kilmolin, co. Wicklow; several of his descendants are buried at Powerscourt, in the same county. At one time there was a baronetcy in the family, for John Sabine, of Eyre, in the parish of Gravenhurst, in Bedfordshire, was created a baronet March 2, 1670. He left no male issue, hence the title became extinct. At an earlier period, in 1649, one of the name, an Alderman Sabine, left a sum of money for charitable purposes to the city of Canterbury.

Sir Edward's great grandfather, Joseph Sabine, had served with great distinction in Marlborough's campaigns, and was rewarded with the Governorship of Gibraltar, where he died in 1739. He purchased in 1715 the property of Tewin (sold in 1810). A great uncle was killed at the battle of Fontenoy.

Sir Edward was born in Great Britain Street, Dublin, October 14, 1788, and his mother, Sarah, daughter of Rowland Hunt, Esq., of Boreatton Park, Salop, died within a month of his birth. The child was taken by his father and sisters to the care of a Mrs. Davies, of Bath, a warm friend of his mother's.

Of his brothers, two made their mark in life. The second, John, was a captain in the 25th Foot, and apparently a good accountant, for he was complimented by the Horse Guards in 1800 for the way in which he had managed the accounts of the regiment. The eldest brother, Joseph, who died in 1837, was a distinguished Fellow of the Society. He was the first secretary to the Horticultural Society, as we learn from the notice of his works, 'Abstracts of the Papers,' vol. iv, p. 15.

At the age of 14, Edward Sabine went to school at Marlow, and being the quickest among all the boys he was called up by the mathematical masters to be examined in order to form the course of instruction at the school.

In January, 1803, he went to Woolwich, where he remained only one month in each class, and obtained his first commission at the early age of fifteen years and two months, in December, 1803, after a little over ten months as a cadet. So urgent was the occasion that the professors and masters were called on to forego their usual vacation to forward the public service.

His subsequent commissions were dated as follows:—Captain,

1813; Major, 1837; Lieutenant-Colonel, 1841; Colonel, 1851; Major-General, 1856; Lieutenant-General, 1865; and General, 1870. He was gazetted K.C.B. in 1869.

After a year at Woolwich he proceeded to Gibraltar, and returning in 1807 was appointed to the Horse Artillery, in which he served at various home stations until the end of 1812. On his promotion in January, 1813, he fell to a company in Canada, and it was on his voyage to Halifax in the "Manchester," Falmouth packet, that he had the first opportunity of showing how gallant a spirit was to be afterwards diverted from the pursuit of military distinction into other channels. The "Manchester," when eight days out, was attacked by one of those American privateers which swarmed at that period to such an extent that they captured one out of every four of the Falmouth packets in 1812. After a running fight of twenty hours' and a close engagement of over an hour, she had to strike her colours to greatly superior force, on the 24th June, in latitude 47° 10′ N., longitude 24° 10′ W. The privateer was the "Yorktown," Captain Rider, a cut-down East Indiaman of 500 tons, carrying nine long 12-prs. and a crew of 116 men, of whom thirty-six served as marines. The "Manchester" had only three 9-pr. carronades, two long and two light 6-prs., with a crew of only thirty-six men, including passengers, and only two could be spared to return the musketry fire of the enemy. That an obstinate resistance was offered with such disparity of strength, that the British packet fought until she had only 10 lbs. of powder left, and the 9-prs. were reduced to firing 6-pr. case-shot, those guns being disabled, was pre-eminently due to the gallantry of Captain Sabine seconding her commander, Captain Elphinstone. "Captain Sabine and his servant" (a gunner of the old school) "were of the greatest assistance to me," wrote the commander, "and the enemy has confessed that my stern chasers, which were pointed by Sabine and a Mr. Bell, passenger, so completely annoyed them, and did them so much damage, that, although greatly our superiors in sailing, they were loth to range alongside." Sabine himself wrote a detailed account of the action, in which he observed: "We had the satisfaction to find, notwithstanding the difference of metal, the 'Yorktown' had received as much material injury as the 'Manchester,' her foremast and foretop-gallant-mast being shot through, and her hull much damaged; the 'Manchester's' mainmast and maintop-mast were struck in several places, and scarcely a rope left whole in the ship, the grape-shot and musketry having been very heavy, especially at the close of the action when we neared one another. The enemy fired high, and our bulwarks were very good, which accounts for only three men being wounded—two with grape and one by a musket-shot. The 'Yorktown's' bulwarks were 14 inches solid timber, which formed a

protection for their men which it was scarcely possible for our shot to penetrate. We knew, however, that one man was wounded, by the doctor's acknowledgment, and had reason to suppose that there were more, from an observation of the lieutenant: 'That three or four young hands had fancied themselves hurt.' We were repeatedly told both by the officers and crew of the privateer that they wished we had never fallen in with them, both in consequence of the expenditure of powder and shot, and of the injury done to their foremast. We fired above 400 rounds. They must have fired many more." He mentions that every gun of the enemy was loaded with a *double-headed* shot and a bag containing sixty grape-shot.

The Americans behaved very well. Sabine commended their discipline. They respected private property, and treated their prisoners with every respect. The master, mate, and crew were transferred a couple of days later to another prize. The captain and passengers remained on board in charge of a prize-master, and with a crew of seven men set sail for New York. As good fortune would have, they fell in with the "Maidstone" frigate on Sunday, 18th July, and were recaptured; the privateer herself having been taken by the "Maidstone" on the previous day. Thus this little episode came to an end. Instead of being landed prisoners at New York, Sabine and his companions were set on shore at Halifax, his port of destination, and after a week's delay he was sent round to Quebec.

It was during his service at Quebec in the winter of 1813–14, that an incident occurred,* which Sabine was fond of quoting in later life, in illustration of the value of sometimes thinking for one's self. Among the little frontier operations of 1814 was an advance of some American militia on Quebec; and he was sent with a small party to garrison an outpost. He was directed to take a 4-pr. gun with him; but finding in the arsenal a light 24-pr. howitzer, which had been left there by General Burgoyne when he went on the Saratoga Expedition, he thought he could manage to transport that; and with or without permission he did so. He found some coehorn shells, and took those, and a supply of fuzes. He reached his blockhouse before the Americans, under a Colonel Williamson, appeared: who, arriving in the evening, were settling in their camp and busy about cooking, when, to their great astonishment, one of these extemporised shrapnels was fired at them with considerable effect. They forthwith retreated to, what they thought, a safe distance; but another shell burst among them, and seemed to throw them into great confusion. They still further retreated, until fairly out of range. In the morning, when Colonel Williamson ordered them to advance to attack the post, a man stepped forward and declared he would

* This incident the writer learned from the late Rev. T. R. Robinson, D.D., F.R.S.

do no such thing, but intended to return home immediately. He was ready to fight to the death in defence of his own country; but he did not see the sense of invading that of others when they had enough of their own. The commander ordered him to be arrested; but, to his dismay, no one obeyed the order, and when he threatened them they said they had all agreed with the spokesman, and would go home—and so they did.*

Captain Sabine's next service was on the Niagara frontier, where he was favourably mentioned in despatches; and was long the last survivor of the battery, and was privileged to wear the word "Niagara" on dress and appointments.

He was in command of the batteries at the siege of Fort Erie in 1814.

In the year 1816 he returned to England, and then he first began to direct his attention to terrestrial magnetism and pendulum observations, at the house of his brother-in-law, Mr. Henry Browne, F.R.S., 2, Portland Place, to whom he was indebted for first directing his thoughts to these sciences. This house was more or less his home for many years; there he met frequently Captain Henry Kater, F.R.S., and became attracted forcibly to similar lines of inquiry; and thence he started on his successive expeditions. He had, however, doubtless studied practical astronomy previously, for in 1818 his reputation as a skilful observer was such that the President and Council of the Royal Society recommended him for the appointment of Astronomer to accompany the Expedition, sent in search of a north-west passage, under Commander (subsequently Sir) John Ross in that year.

His attention, however, was not solely directed to physics, for his report on the biological results of the expedition appeared in vol. 12 of the 'Transactions of the Linnean Society,' and it embraced twenty-four species of birds of Greenland, of which four were new to the list, and one, the *Larus Sabini*, described by his brother Joseph, entirely new.

On his return Sabine was not long allowed to be idle, for on the equipment of a second expedition in 1819, under Lieutenant-Commander (subsequently Sir Edward) Parry, he was again selected to accompany it.

Parry, in the introduction to his Journal, published 1821, acknowledges Sabine's labours in these terms:—

"The various observations made on board the 'Hecla' during the voyage, have been carefully collected into tables, on the model of those of Wales and Bayly, by Captain Sabine, to whom I am indebted for the arrangement of nearly the whole of the Appendix, and for the

* Captain Sabine learned these details afterwards in New York from some of the officers present.

superintendence of that part of the work during its progress through the press. I feel it no less a duty than a pleasure to acknowledge that in the performance of this task, Captain Sabine has added another to the many obligations I owe him for his valuable advice and assistance during the whole course of his voyage, to the credit of which his individual labours have so essentially contributed."

It might have been added that he contributed not a little to the cheerfulness and harmony of the expedition, by consenting to edit the 'North Georgia Gazette and Winter Chronicle,' a weekly publication established during its tedious stay at Winter Harbour, where the sun was ninety-six days below the horizon. It extended to twenty-one issues. The total strength of the expedition being but eighteen, the editor had to rely very much on his own pen. The result was so highly appreciated as to call for republication.

Sabine did not accompany Parry on his second voyage, the post of astronomer being filled by the chaplain, the Rev. George Fisher. He was himself selected to conduct a series of experiments for determining the variation in the length of the pendulum vibrating seconds, in different latitudes, a subject which had engaged his attention in the first voyage.

Sabine in the pursuit of this investigation visited St. Thomas (Gulf of Guinea), Maranham, Ascension, Sierra Leone, Trinidad, Bahia, Jamaica, in 1821–22; New York, Trondhjem, Hammerfest, Greenland and Spitzbergen in 1823. No less than five men of the Royal Marines, who had been placed at his disposal by Captain Clavering, R.N., as assistants at Sierra Leone, were carried off by fever during a stay of about five weeks in that pestilential climate.

Sabine's observations of the "magnetic" inclination and force at St. Thomas in 1822 were probably the first ever made on that island. Utilised as a base of comparison with the recent magnetic observations of the Portuguese, they become important in showing the remarkable secular change that has been in progress in those elements during the interval.

The remarkable account of his pendulum experiments, printed in a 4to. volume by the Board of Longitude in 1825, must always remain an enduring monument to his scientific merit. It would be impossible in this brief notice to give any adequate idea of the indefatigable industry, the spirit of inquiry, or the range of observation evinced. The work was honored by the Lalande Gold Medal of the Institute of France, in 1826.

The subject of pendulum experiments was one in which he long took great interest; in the years 1827 to 1830 he made experiments to determine the relative lengths of the seconds pendulum in Paris, in London, in the Royal Observatory at Greenwich, and at Altona, and he afterwards determined the absolute length at Greenwich.

Finally, in 1864, he moved the Government of India to undertake the series of pendulum observations at various stations of the Great Trigonometrical Survey, from the sea level at Cape Comorin to the lofty tablelands of the Himalayas, which have thrown so much light on the constitution of the earth's crust, and on local variations of gravity.

The year 1826 was marked by his happy union with Miss Elizabeth Juliana Leeves, daughter of William Leeves, of Tortington, thenceforth his inseparable companion, his invaluable and devoted assistant, and latterly his vigilant guardian, until their union was dissolved by her death, in 1879, after fifty-three years of married life. Her mastery of the German language was something exceptional. She made herself competent to share in every investigation her husband undertook, and habitually examined and checked his work; suffice it to say that thenceforward his powers were doubled.

Captain Sabine's next service, after the publication of his pendulum experiments, was as a Joint Commissioner, in 1825, with Sir John Herschel, to take part with a Commission, nominated by the French Government, to determine the precise difference of longitude between the observatories of Paris and Greenwich, by means of rocket signals. Herschel and M. Largeteau were the observers on the French side of the Channel at Lignières, Captain Sabine and Colonel Bonne on the British side, at Fairlight Downs, near Hastings. The difference of longitude thus found was 9 m. 21·6 s., which was believed to be not more than one-tenth of a second in error, and extremely unlikely to prove erroneous to twice that amount. The accepted difference at the present day of electrical signalling is 9 m. 21·0 s., a slightly larger error, but the determination was very close. In 1827, he compared the length of the seconds pendulum and the magnetic force of the earth at the same two stations.

Having obtained from the Duke of Wellington, then Master-General of the Ordnance, a general leave of absence from military duties, as long as he could usefully be employed in scientific pursuits; he became in 1827 one of the secretaries of this Society, to which he had been elected a Fellow in 1818. This office he filled down to 1829.

The condition of Ireland in 1830 was, as it has been usually since, one to occasion the gravest anxiety. There was a failure of the potato, from the effects of a cold wet summer; local famines; an epidemic of influenza; constant collisions between the peasantry and the police. Under such circumstances Captain Sabine was ordered to join his company: he served with it, or occasionally on the Staff of his friend General Sir James Douglas, K.C.B., for the ensuing seven years, and acquired the character of being a very "smart officer:" but the time was by no means lost to science. He published

two Pendulum papers in 1830 and one in 1831. In 1834 he commenced, in conjunction with the Rev. Humphry Lloyd (afterwards Provost of Trinity College), and Captain James Clarke Ross, R.N. (afterwards Sir James Ross, of Antarctic fame), the first systematic magnetic survey ever made of the British Islands. He extended it single-handed, to Scotland, in 1836, and to England, in conjunction with the same and some additional observers, in the following year. With the exception of the mathematical section of the Irish Report, which was Professor Lloyd's, the Reports—which were published by the British Association—were mainly his; and also a very large share of the observations, more particularly the laborious task of combining them, by equations of condition, to obtain the most probable mean results. About twenty-three years later (1858-61), with unabated interest, and still privileged to number Dr. Lloyd among his coadjutors, he undertook, at the request of the General Committee of the British Association, to repeat this Survey; and, as before, reduced and reported the results, as far as concerned the elements of Dip and Force. Captain (afterwards Sir F.) Evans, R.N., Hydrographer to the Admiralty, dealt with the Declination.

The year 1836 will ever be memorable in the history of British Science, for a letter, dated 22nd April, addressed to H.R.H. the Duke of Sussex, President of the Royal Society, by Baron Alexander von Humboldt, in which he referred to conversations he had held with Sabine and Lloyd, on a recent visit which they had paid to Berlin, and he urged upon the British Government to establish regular magnetical stations in different parts of the British Empire, similar to those which, mainly through his influence and exertions, had already been some years in operation in Northern Asia. This letter was referred to the Astronomer Royal, Mr. (afterwards Sir George) Airy, and Samuel Hunter Christie for a report.—'Roy. Soc. Proc.,' vol. 3.

A Committee on Mathematics and Physics was appointed in May, 1838, of which Major Sabine and Professor Lloyd were prominent members, and towards the end of the year the definitive and official recommendation was made to H.M. Government, already prepared to receive it, to establish magnetic observatories at selected stations in both hemispheres, and despatch a Naval Expedition to the Southern Hemisphere, with the purpose of making a magnetic survey of the Antarctic regions.

It is needless to say that Major Sabine played an active and conspicuous part in all these negociations and preparatory arrangements.

The observations were placed under the charge of Lieutenants of the Royal Artillery, all of whom went through a course of preliminary training at the Magnetic Observatory, Trinity College, Dublin, under the superintendence of Professor Lloyd; in fact, the scientific supervision of the scheme was left in great measure in Lloyd's hands.

The first three officers invited to join the scheme were: the late Lieutenant-General Sir J. H. Lefroy, the late Lieutenant-General F. M. Eardley Wilmot and Major-General C. J. B. Riddell. These were soon followed by the late General W. J. Smythe, Lieutenant-General C. Younghusband, Major-General H. Clerk, and the late Lieutenant-Colonel H. F. Strange. The naval expedition was placed under the command of Captain (afterwards Sir James Clark) Ross.

The observatories began their work in 1840. The first publication was a 4to. volume of observations on days of unusual magnetic disturbance, published in 1843, which was followed by a second, on the same subject, in 1851.

The subsequent publications are dated as follows :—

Toronto	to 1842, Vol. I.	1845.
,, ...	to 1845, Vol. II.	1853.
,, ...	to 1847, Vol. III.	1857.
,, ...	The observations from 1848 to 1853 remain in MS.	
St. Helena..................	to 1843, Vol. I.	1850.
,, ...	to 1849, Vol. II.	1860.
Cape of Good Hope, magnetic..	to 1846, Vol. I.	1851.
,, ,, meteorological	to 1848, Vol. II.	1880.
Hobarton, Tasmania..........	to 1842, Vol. I.	1850.
,, ,, ...	,, Vol. II.	1852.
,, ,, ...	,, Vol. III.	1853.

Sir E. Sabine was enabled to complete this enormous amount of work by the fact that for upwards of twenty years a clerical establishment was maintained by the War Office at Woolwich, under his own special control.

From these official publications, we pass to Magnetic Surveys. Sir E. Sabine lived to complete in fifteen "Contributions" to the 'Philosophical Transactions,' a gigantic work undertaken by him, namely, a survey of the general Distribution of Magnetism over the Globe at this epoch. Several of these appeared after he had lost the aid of his establishment of clerks, the last in 1876. In these are to be found every observation of any authority, taken by sea or land, since 1818, or thereabouts, arranged in zones of 5° and 10° of latitude, and taken in the order of longitude eastward from Greenwich round the globe. All the results of Arctic and Antarctic voyages, and special expeditions were utilised. Everything that he could glean from Russian, German, American and other foreign sources, much of which is not accessible in any other form; and these were accompanied by maps prepared for him in the Hydrographical Department of the Admiralty, under the supervision of Captain (subsequently Sir) Frederick Evans, R.N. It may be safely said that had Sir E. Sabine

done nothing but collect, compile, and discuss this vast mass of material, he would have rendered a service to science, which would make his name live as long as Halley's, but it forms only a part of his life labour. Communications to this Society and the Philosophical Magazines, on some subject of the moment, were always flowing from his pen. His addresses to this Society and to the British Association, as President, must not be forgotten. Our Catalogue down to 1874 contains 101 titles of papers by him, and his activity did not cease with that year.

Sir Edward's military honours have been already enumerated, as well as the fact of his temporary service as Secretary of this Society, 1827–29. His subsequent appointments were as follows:—In 1839 he was elected General Secretary of the British Association, a laborious office which he continued to hold for twenty years, with the single exception of the year 1852, when he exchanged the Secretaryship for the Presidential Chair, at the first meeting at Belfast.

In 1846 he was elected Foreign Secretary of this Society, in 1857, its Treasurer, and finally, four years later, he was chosen President, an office which he held till 1871.

In 1821 he received the Copley Medal; in 1826 the Lalande Medal of the Institute of France, and in 1849 one of our Royal Medals. Of foreign orders he held that of *pour le Mérite* from Prussia, SS. Maurice and Lazarus from Italy, and the Rose from Brazil.

Turning to another view of his character, it may be said that Sir Edward's scientific capabilities were heightened by his social qualities. His grace of manner, his cheerful voice, and his brightness of aspect impressed all who came within his influence. A Fellow of the Society, who had travelled up from Manchester to be present at one of the Conversaziones, once remarked, "It is worth all the time and trouble to see, on arrival, the President's smile."

In the year 1876, his scientific activity came to an end. In 1879 he lost his wife, who for more than half a century had found her chief happiness in placing at the service of his scientific investigations the best efforts of a mind and a memory such as rarely have been given to any woman.

He himself finally passed away, at Richmond, June 26th, 1883, at the patriarchal age of 94 years 8 months. He was buried quietly at Tewin, Herts, in the vault belonging to his family, and beside the remains of his wife.

Sir Edward left no issue, and the very name of Sabine in the direct line of his family has become almost extinct, for his only surviving nephew on the male side, the late Admiral Sir Thomas Sabine Pasley, K.C.B., had taken the additional name of Pasley.